VOLUME XLIX NUMBER 5

BOTANICAL GAZETTE

MAY 1910

CHARLES REID BARNES

(WITH PORTRAIT)

CHARLES REID BARNES was born at Madison, Indiana, September 7, 1858, and died at Chicago, from an accidental fall, February 24, 1910. He graduated at Hanover College in 1877, and afterward studied at Harvard University, where he secured the friendship of Professor ASA GRAY. After teaching in the public schools for a few years, he became professor of botany at Purdue University in 1882. In 1887 he was called to the University of Wisconsin, and for eleven years developed and maintained a vigorous department of botany in that growing institution. In 1898 he became professor of plant physiology at the University of Chicago, and completed twenty-eight years as a university professor. At Hanover College he met Professor COULTER as his instructor in botany, and from that time they became intimately associated, first as joint editors of the BOTANICAL GAZETTE, and later as colleagues in the same university.

He was always active in scientific societies, and the esteem in which he was held by his colleagues is indicated by the positions he held. He became a member of the American Association for the Advancement of Science in 1884 and a fellow in 1885; was secretary of the botanical section in 1894, secretary of the council in 1895, general secretary in 1896, and vice-president (chairman) of the botanical section in 1898, giving his retiring address at Columbus in 1899 on "The progress and problems of plant physiology." He was secretary of the Botanical Society of America from its organization in 1894 to 1898, and became its president in 1903, giving his retiring address at Philadelphia in 1904 on "The theory of respiration." In 1905 he was a delegate from the botanical section of the

American Association to the International Botanical Congress at Vienna.

As a teacher Professor BARNES had few equals. There was a clearness and precision in his statements that left nothing to be desired. This power of presentation was reinforced by a personality so winning, on account of its brightness and friendliness, that students were attracted not only to the subject, but also to the man. This is teaching at its highest level, and his many students throughout the country are feeling the loss of a friend of powerful personality even more than of a teacher of unusual ability. His power of training men was conspicuous, and his critical sense was an unusually valuable asset in a department active in research. An investigation or a thesis which had run the gauntlet of his frank and keen criticism was equipped to face the public.

In 1883 Professor BARNES became coeditor of the BOTANICAL GAZETTE, and for twenty-seven years filled this position with remarkable efficiency. He had the editorial genius, which entered into every detail, from general policy to printing. He was complete master of all the details that belong to such work, and was continuously solicitous as to form, accuracy, and high standards of every kind. He was especially expert in the work of illustration, and with remarkable patience corrected the blunders of inexperienced or careless contributors. The laborious work of editing manuscripts and illustrations obtains little or no public recognition, but in his work as a reviewer Professor BARNES achieved high reputation. He grasped the significant things and presented them with a clearness and a force that is unusual. Moreover, he felt keenly his responsibility to the readers of the journal and to his science, and let no doubtful results or inferior work slip by without incisive comment.

As a writer Professor BARNES was not voluminous, but very effective. He never wrote for the sake of writing, but because he had something to say. He had a style and a grasp that are sadly lacking in most scientific writing today. He felt that clear statement comes naturally from clear understanding, and that muddy statement throws suspicion on the understanding. When the organizing instinct of the teacher deals with clear and sharp-cut statements, the result is a presentation that is a real contribution not only to knowl-

edge, but also to good writing. Aside from his work as editor and reviewer, his botanical writings fall into four categories.

1. TEXTBOOKS.—Professor BARNES played an important part in the development of laboratory work in this country. The spirit of the *Lehrbuch* of SACHS had been introduced into this country by Professor BESSEY, but many teachers at that time needed specific directions for studying the material called for. To meet this need, the then editors of the BOTANICAL GAZETTE, Professors ARTHUR, BARNES, and COULTER, prepared the *Handbook of plant dissection*, which was published in 1886. The accident of the names of the joint authors and the sequence of their parts in the book led to its informal designation as "the A B C of botany." Through the whole period of laboratory organization and development this book played its part as a guide, and through it the teaching power and technical facility of Professor BARNES reached the teachers.

In 1898 he published his *Plant life*, a textbook for secondary schools. This was about the first text in this country that recognized physiology and ecology as subjects for study in secondary schools, and was considerably in advance of the preparation of the teachers. A briefer edition appeared in 1900, entitled *Outlines of plant life*.

His ripened experience as a teacher and an investigator had just expressed itself in a textbook on plant physiology for college use. He was permitted to read the final proofs, so that the work will appear just as he had it in mind. This will stand as a permanent record of his point of view, of his characteristic and telling way of putting things, of his critical analysis of difficult problems and doubtful situations.

2. TAXONOMY.—Like almost every American botanist whose training began thirty years ago, his first contact with the subject was through the GRAY texts, and his first interest was in local floras. Various small papers were published, but soon the mosses attracted his attention as a group needing investigators at that time. Perhaps his first general recognition among botanists came through his taxonomic work on this group, the most important publications being *Analytic key to the genera of mosses* (1886); *Revision of the North American species of Fissidens* (1887); *Artificial keys to the genera and species of North American mosses* (1890), revised in col-

laboration with HEALD in 1897; and a series entitled *Notes on North American mosses* in the BOTANICAL GAZETTE, leading to a revision of Dicranum in collaboration with TRUE.

3. PHYSIOLOGY.—The chief contributions of Professor BARNES in plant physiology were as a critical reviewer, a teacher and effective guide in research, and a sponsor for certain points of view. He was an early champion of the restricted use of the term plant food now generally held; advocated many years ago the use of the term photosynthesis (or photosyntax), in place of assimilation, for the first stages of food-making; and consistently advocated the restriction of sex terms to sex organs. The vice-presidential address of 1899, and even more the presidential address of 1904, gave stimulating points of view much in advance of current usage. To few is given the ability to make so clear the actual status when the subject is foggy and uncertain, as is the case in so many regions of physiology.

4. MORPHOLOGY.—After his removal to Chicago, Professor BARNES became greatly interested in the special morphological problems presented by the bryophytes, and for a number of years offered courses in the subject, in cooperation with Dr. LAND. In connection with this work, Drs. BARNES and LAND made extensive field studies and collections in Mexico in 1906 and 1908. There had already appeared two joint papers, one on *The origin of air chambers*, and the other on *The origin of the cupule of Marchantia*. Several other joint papers are in various stages of preparation, and are to be issued by the junior author. It was planned that this series of studies should lead to the preparation of a general work on the special morphology of bryophytes.

The intellectual horizon of Professor BARNES swept in a much larger circle than his professional subject. No man had wider interests, or brought to them a saner mind. There was no assurance or dogmatism on the one hand, or no wabbling uncertainty on the other. The whole mental attitude was judicial, one of perfect poise, friendly to truth from every direction; and the judgment was clean cut, but never final. His loss to his department, his university, and his science is irreparable, for although institutions and subjects outlive their men, a resource that may be replaced in amount, but never in kind, has now become a memory.

PHYSIOLOGICALLY ARID HABITATS AND DROUGHT RESISTANCE IN PLANTS[1]

ALFRED DACHNOWSKI

In the course of the investigations on the ecology of a local bog, the trend of the work has invited a closer consideration of a number of points of interest to a physiologist. The importance of these in life processes has been the occasion to offer, in the following, a statement of at least the more special cases.

In previous papers (this journal) attention has been called to the fact that the physiological effect of bog water and bog soil to a great extent plays an important primary rôle in the determination of the flora which can best succeed in bogs. It was shown that a number of cultivated plants and plants from different but neighboring habitats in the same locality fail to develop normally when grown in the bog or under laboratory conditions, and that the toxicity of the habitat appeared, therefore, to exert a marked influence in determining not only the character but also the distribution of plants within the same habitat. A difference was shown to exist between different species in their power of resistance to the toxic action of the substratum, so as to leave no doubt that some species of plants are better adapted than others to growing in soils containing relatively large amounts of these toxins. It was further shown that in the effect of a salt of calcium, and especially in the presence of a number of insoluble adsorbing bodies, these differences are much less pronounced, and that the substances tend very greatly to diminish, not only the difference between different species as to their tolerance, but also the differences in physiological aridity existing between different zones within the same habitat. With the establishment of certain definite relations between toxicity and physiological behavior of plants, it seemed desirable to study somewhat more closely the nature of the resistance in bog plants to toxicity, and consequently to physiological drought.

One of the most effective methods of dealing with the problem,

[1] Contribution from the Botanical Laboratory of Ohio State University, 55.

and one which is particularly apropos at this time, is the study of the character and the development of drought resistance in bog plants. This should naturally be the first consideration. Too little experimental information is available in the whole field of plant responses, whether as adjustments or adaptations, and our knowledge of plant problems connected with resistance in plants to sterile soils or to unfavorable climate is particularly limited. The modifications of structure which can be attributed to the action of these and other environmental factors are well known, and may be referred to structures serving to reduce the evaporation of water from leaves. The necessity for such protection in bog plants is the greater, not on account of the fact that the vegetation is directly exposed to the drying effect of wind, to lower humidity, and to stronger light, but because roots absorb water with difficulty when it contains any considerable percentage of toxic ingredients. Unless bog plants differ from other plants in some phase of root function, the amount of transpiration must be kept low by structural modifications, that is, in order to compensate a reduced absorbing activity of the roots, the escape of water from the shoots must be correspondingly checked. It is unnecessary here to cite in detail the structural modifications in bog plants that are effective in diminishing transpiration: the general reduction in size of the leaves; the thick-walled epidermis and hypodermal tissues, reinforced by cuticle, wax, and hairs; the mucilaginous and resinous bodies found in roots and leaves; and the prevailing grasslike form are well-known characteristics. But much of the local bog vegetation exhibits little or no xerophytic structure. In some places the outermost growth which immediately borders the open water is, indeed, for the most part hygrophile. It is dominated, in part, by *Typha latifolia*, forming sometimes a nearly pure association. *Sagittaria latifolia*, *Pontederia cordata*, *Peltandra virginica*, *Decodon verticillatus*, *Polygonum hydropiperoides*, and others are ordinarily abundant with Typha. The xerophytic quality is least marked in this vegetation. Differences such as these must arise strictly from the edaphic causes, which seem to set a natural and inevitable limit to what may be accomplished through invasion. There is but one important point of difference between the environment of the bordering vegetation and that of the bog plants occurring

more centrally on the island, and that is the relative absence of toxic ingredients in the water and in the upper limits of the substratum. The transpiration quantity of such hydrophytic plants is correspondingly less when grown in bog water from the central habitat. The low rate of transpiration can be increased again with the addition of adsorbents, or with the dilution of the bog water to a point no longer fatal in its effects. The peat substratum of the border zone permits, on that account, a dense luxuriant growth of hydrophytes, and neither a smaller percentage of free oxygen in the water, nor the absence of a mineral soil, nor the incoherency of the substratum affords precarious conditions for growth.

How much of the salts dissolved in the lake water is retained by absorption in the humus soils along the margin of the bog has not been determined. There is usually a slight difference between the total mineral content of bog water and that of the lake water adjoining. The figures obtained, however, are not high. The osmotic pressure and the acidity have been found to be the same for both habitats. The stress laid by various authors upon the relation of these two factors to plant societies in bogs, in so far at least as this region is concerned, will not hold. They are not the factors in the selection and distribution of species.

Water culture experiments are here considered as giving a relatively normal indication of conditions obtained in the bog, since the bog island itself is merely a water culture on a larger scale, with the mineral soil at a depth of more than 10^{m} from the surface vegetation. In the bog habitat of the island, however, the ratio between the amounts of water absorbed and transpired is never constant. It varies most during the growing season, and yet it must be always more than unity, if the plants are to survive the occasional periods of extreme physiological drought during the summer and autumn months. This is evident in the scattered small trees of *Acer rubrum*, *Rhus vernix*, and in *Cephalanthus occidentalis*, *Osmunda cinnamomea*, and other plants from the adjoining relatively forested maple-alder zone. The stunted growth of these plants, their numerous dead branches, and ragged crown of foliage exhibit to a marked degree the physiological action of the substratum. The resistance offered by the invading mesophytic vegetation is, indeed, but slightly

effectual. The relative power in the bog plants for absorbing or rejecting the various injurious constituents of bog water and bog soil is here the controlling factor. This regulatory compensation within certain limits, therefore, is of utmost importance in these species, for we have here an instance of the fact that while the presence of structural modifications is generally regarded as a circumstance in favor of a bog vegetation, the most noteworthy characteristic is the ability to neutralize the injurious action of the substratum. In so far as the adjustments arise through resistance to toxicity and consequent drought, one is painfully aware that neither the nature of the drought resistance, its origin, its specific governing factors, nor the specific type of resistance involved in the adaptation of plants to toxic bog conditions is known.

Another matter of fundamental importance is the degree in which this drought resistance in physiologically arid habitats varies, and the extent to which it is inherited. That activities are not constant but variable in degree and in kind, and that functional characteristics in plants are transmitted as effectively as habit of growth, form, flower, and seed, is generally admitted. The conditions of life in bogs are always active in stimulating or depressing normal functions, and they are not without effect upon their degree and character as well. The disturbing effect of adverse conditions, if not too severe, may be gradually overcome through variations in the degree of activity. Often the unfavorable external conditions are conducive to the development of a new *place-junction*, and the immunization may extend to the poi nt that would be fatal at the outset. Thus plants reared in a gradually intensified toxic solution have reached a power of resistance above that of the organisms under normal conditions. It seems possible to raise forms in which the special resistant power becomes a permanent hereditary character. Phenomena such as these are remarkable for what they suggest rather than for conclusions that can be positively drawn, since the degree of resistance which plants acquire by external or other influences to poisons and drought, or to other adverse conditions of life, may be one in which the activities are modified, resulting, as has been stated, in a new place-function, or one in which the plants succeed in resisting the changed conditions through greater elasticity in functions. A

knowledge of the limits of functional variation within species, therefore, is very essential. Deviations in the very functioning of the process, and in transmission as well, cannot but be fruitful in a greater efficiency of responses in plants, and without doubt these are the differences that go far toward constituting the essential difference between relative power of endurance and resistance against drought, disease, or abnormal conditions in any plant. The circumstances under which bog plants can function unaltered, either in character or degree, and the limits beyond which such functions must cease or become altered, are questions of special significance in physiological ecology. They throw no little light upon the nature of tolerance and drought resistance, a topic upon which information is sorely needed if we are to regard functions no less important in the light of development and heredity than form or structural organization.

Of still greater consequence is the study of such limiting factors of soil and climate as operate to preserve adapted forms, or eliminate species in which the responses fail to secure better adaptation to the factors provided by a local region. The presence and fitness of bog plants and of invading hydrophytes and mesophytes resistant to physiological drought seem to be due mainly to more efficient responses to edaphic conditions. In most cases, at least, drought resistance involves a specific reaction on the part of the plant to more than one factor of the physical and biotic environment, the ratio between the factors in the process being more significant than the structural modifications or the regulatory compensation which may be possible within the plants. The reaction may be in part one of structural differences, and in part one of endurance. The evidence cited in the work published, and experiments now in progress indicate that drought resistance in physiologically arid habitats is due to a specific protective functional root activity. It is fundamentally a chemical reaction, but one too complex to solve by ordinary methods of chemistry. The delicacy of the reaction may be better understood if we recall the fact that it is adjusted to meet a specific limiting condition. Whether the toxic action of bog water and bog soil is determined by the action of one constituent, or by the combined action of several, needs more detailed study. As has been noted elsewhere, the toxicity of any bog soil is but a function of the number of the con-

ditions of its development, and of the physiological efficiency of these factors.

Recent work of an experimental nature with cultures of isolated bog bacteria has shown that the injurious products of the bacterial bog flora accumulating in definite layers of the peat substratum are the leading factor to be considered in the physiological aridity of the local study. The observation was made that a sample of bog water in a well-corked glass jar, when kept in the dark at 20–25° C., soon shows the formation of methane and other gases, and that in a few weeks a thin surface layer becomes noticeable made of up bacteria imbedded in a matrix. Exclusion of air prevents further growth of aerobs, for in flasks with narrow necks and closed with a cotton plug a surface growth is never evident. The breaking down of organic matter by bacteria seems to involve, therefore, not only the growth and multiplication of the agents of decomposition, but also an accumulation of by-products which soon decreases the rapidity of aerobic bacterial action. Facts to justify the position as to the presence of a constituent that imparts an antiseptic action to bog water and bog soil are still lacking, but there is sufficient experimental evidence to justify the statement that the bacterial flora has the greater share, not only in the formation of these toxins, but also in the slow and partial decay of bog and swamp plants, that is, in the formation and preservation of peat. An attempt has been made to study the physiological reaction of the products formed from the activity of single isolated species, as well as the effects of the residual products due to mixtures of bog bacteria. Without going into too much detail it is sufficient to point out here the following:

From a fresh sample of bog water and bog soil dilution cultures were prepared. The isolation of the various species was continued upon peat-agar plates, and later in test tubes containing a beef-broth-agar medium, until from the bacterial colonies which appeared upon them, pure cultures were obtained. About 35 different species of bacteria have thus far been isolated. The organisms belong for the most part to the aerobs. Of the bacteria thus isolated, 21 were tested for their toxin-producing power upon a sterilized solution of bog water and peat. A number of flasks of a liter capacity containing the sterilized solution were then inoculated with the respective pure

cultures. Several flasks were left sterile to serve as controls, while others were inoculated with a mixture of the various species and with the bacteria found in 1^{cc} of fresh bog water. An additional test condition was arranged at the same time from the normal untreated bog water. All flasks were placed in an incubator for a period varying from two to four or six weeks, and at such times were then brought to the laboratory. All physiological tests were made in duplicate series, and the greatest caution was observed to reduce the dangers of contamination during the preparation of the cultures. Wheat seedlings germinated in sterilized quartz and carefully washed in distilled water were used as indicators. They were transplanted to half-liter "Mason" jars in a manner as described in previous papers.

From the data at hand the following have been selected to illustrate the variation in virulency of bacterial products. Using the transpiration of the controls as a basis, and representing it as 100, the different bacterial cultures have values in the order as indicated in the last two columns of the tables below.

These figures show conclusively that in all cases the bacteria are responsible for the proportionally diminished transpiration and growth of the wheat plants. In all cultures the transpiration values lie below that of the control, varying from 20 to 52 per cent. The evidence derived from the duplicate series is omitted in tables II and III, showing as it does results closely parallel.

More recent experiments with pure and mixed cultures of bog bacteria confirm the earlier experiments as to the ability of these organisms to increase the amount of toxins present in the peat substratum. These results are significant in that they demonstrate the active participation of microorganisms in bog habitats, a fact contrary to the position taken by FRÜH and SCHRÖTER, who regard the bacterial influence as very small. To what extent the tables suggest the possibility that bacterial diagnosis when correlated with physiological criteria may determine the crop-producing power of different agricultural soils need not be discussed here.

Chemically peat is made up of substances which are commonly classified among the dehydration products of the carbohydrates. The analyses which have been made agree in showing that the phosphoric acid content of peat soils is high and partially in an available

condition for plant use. In potassium content they are deficient; potassium salts are generally added in an alkaline form in order to produce crops. In nitrogenous materials peat soils are unusually

TABLE I

TRANSPIRATION DATA FOR SOLUTIONS INOCULATED WITH PURE CULTURES OF BOG BACTERIA

SERIES III	BACTERIA	TRANSPIRATION IN GRAMS				COMPARATIVE TRANSPIRATION	PERCENTAGE OF DECREASE
		5th day	10th day	15th day	Total		
	Control	9.33	42.92	66.85	119.10	100	0
	B. 20	8.85	41.30	44.06	94.21	79.10	20.90
	B. 22	8.30	38.15	42.90	89.35	75.02	24.98
	B. 7	8.55	31.80	42.80	83.15	69.81	30.19
	C. 3	7.15	30.90	43.95	82.00	68.85	31.15
	C. 4	7.60	29.70	44.40	81.70	68.59	31.41
Duplicate series....	Control	8.80	44.50	66.83	120.13	100	0
	B. 20	8.40	34.25	45.98	88.63	73.77	26.23
	B. 22	7.05	35.40	46.10	88.55	73.71	26.29
	B. 7	8.15	34.45	42.21	84.81	70.59	29.41
	C. 3	8.10	30.90	44.25	83.25	69.30	30.70
	C. 4	8.40	31.15	41.65	81.20	67.59	32.41
Atmometer..		102 gr.	136 gr.	125 gr.			

TABLE II

TRANSPIRATION DATA FOR SOLUTIONS INOCULATED WITH PURE CULTURES OF BOG BACTERIA

SERIES VII	BACTERIA	TRANSPIRATION IN GRAMS				COMPARATIVE TRANSPIRATION	PERCENTAGE OF DECREASE
		5th day	10th day	15th day	Total		
	Control	17.65	36.20	36.60	90.45	100	0
	Bog water	7.65	11.30	8.90	27.85	30.79	69.21
	B. 25	18.15	29.30	26.85	74.30	82.14	17.86
	Bg. w. 1cc	18.27	30.15	25.70	74.12	81.94	18.06
	B. 13	15.72	24.65	30.85	71.22	78.74	21.26
	B. 2	17.45	29.05	24.30	70.80	78.27	21.73
	B. 1	16.60	28.95	24.85	70.40	77.83	22.17
	B. 27	12.60	24.90	22.80	60.50	66.66	33.34
	B. 6	14.00	25.40	20.80	60.20	66.55	33.45
	B. 4	14.95	23.80	20.45	59.20	65.46	34.54
	B. 29	11.60	15.55	15.85	43.00	47.54	52.46
Atmometer..		114 gr.	117 gr.	102 gr.			

rich, but the nitrogen remains bound up in organic compounds unavailable for the growing cultivated plants. In order to determine the ability of the microorganisms to convert soluble proteids into amido-acids and allied products of the disintegration of proteids,

enough pepton was added to sterilized solutions to make an equivalent of a 1 per cent pepton culture. After sterilization the solutions were inoculated with the bacteria indicated in table III. The cultures were tested physiologically at the end of a two-week incubation period. Since the danger of contamination in pepton cultures becomes increasingly greater, the transpiration figures for only the first five days are tabulated. The wheat plants had grown in each experiment for three days when they were photographed.

TABLE III

TRANSPIRATION DATA FOR 1 PER CENT PEPTON CULTURE SOLUTIONS, INOCULATED WITH BOG BACTERIA

Series IX	No.	Bacteria	Transp. (5th day), in grams	Comparative transpiration	Percentage of decrease
	6	Control	17.65	100	0
	7	Pepton check	7.00	39.65	60.35
	13	B. 13	4.85	27.47	72.53
	14	B. 25	2.70	15.30	84.70
	11	B. 2	2.30	13.03	86.97
	12	B. 4	2.40	13.60	86.40
	15	Bg. w. 1^{cc}	1.87	10.60	89.40

A brief inspection of the figures suffices to show that transpiration, growth, green and dry weight of plants are in this case proportionally reduced. The mixed culture solution (Bg. w. 1^{cc}) in which the percentage decrease in transpiration is as low as 90, seems to show that it is the function of some of the bacterial organisms to do the initial work of rendering soluble the protein compounds in a peat substratum. The process of denitrification is carried on up to a point where the products become further decomposed by other organisms. A whole series of bacteria, therefore, seems to be involved, to which are due the residual products, the sum of which in part constitutes the toxicity of the habitat encountered on Cranberry Island, the formation of methane gas, and the reactions which form the basis of the process of humification.

At the end of the experiment a chemical examination of these culture solutions indicated the presence of indol, ammonia, and various non-volatile products in various proportions. A marked difference was noted in the ability of the different species of bacteria to produce indol and ammonia. The highest quantity of ammonia

was produced by B. 13; the least amount was recorded for Bg. w. 1^{cc}, the culture solution, it will be remembered, which consisted of a mixture of the bacteria found in 1^{cc} of fresh bog water. None of these products were found in the control (sterilized bog water and peat). It is also to be noted that in the various pure bacterial cultures neither the organic acids nor the ammonia underwent a further change; in the mixed cultures, however, the decomposition continued, the amido-acids being formed to ammonia and to compounds of the fatty acid series. The extent to which the latter substances may be still further disintegrated to carbon dioxid, hydrogen, and methane is now being determined. Thus far the isolation of bacteria involved in the decomposition of carbohydrates has not been successful. Certain microorganisms have been found to possess the ability to dissolve cellulose (filter paper) in the presence of air. To what extent these forms and the anaerobs play a rôle in the relation of deleterious products in the soils and the cultivation of crops is still uncertain. In a previous paper (BOT. GAZETTE **47**:400. 1909) the writer has reported that the poisonous matter injurious to plant growth is present in the agricultural soils used as filters for bog water. The retardation seen in the contaminated soils lacked the corresponding control average in dry weight of plants to an amount of 18 per cent, 3 per cent, and 36 per cent, for sand, clay, and humus soil respectively. It was further shown that the absorption and retention capacity of soils for toxins became generaliy higher the greater the content in humus.

In concluding this part of the discussion it is well to note the extent to which the results show clearly that, in the laboratory cultures at least, the retardation in growth of wheat plants is not caused by physical or chemical conditions, but through the direct activity of the bacterial flora upon the peat substratum.

It has long been suspected that a reciprocal relation exists between groups of soil bacteria and the plants growing upon the soil. Various writers have been able to point out that marked differences in the productive power of different soils followed the growth of wild plants, and that these differences persist for some time. It is generally concluded, therefore, that the injury caused to cultivated plants by weeds or previous crops is due to influences on the bacterial life in the soil,

and in a direction unfavorable to succeeding agricultural crops. No doubt, the "exhaustion" of soils which is frequently met with, and which cannot always be attributed to the removal of plant nutrients is, in part, an allied phenomenon. It cannot remain a matter of indifference to physiological ecologists whether a strong, intimate, and controlling relation exists between soil bacteria and surface flora, and how the bacterial organisms affect the character, and the association and succession of plants. At best very little is known of this phase of the process, and of the reactions and effects of the bacterial products upon plant life. It would be idle, also, to expect that the bacteriological data in themselves are sufficient for a clear interpretation of toxicity and drought relations, or of sterility of soils. If attempted, the interpretation would be, indeed, one-sided; there is a coordination of factors, each and all of which exert a relatively marked influence. Temperature, water, and air conditions in the soil, as well as the physical and chemical character of it, and the surface flora play an important rôle in determining the character of the bacterial flora, and therefore also the character of the chemical products formed. The bacteriological-chemical, as well as the physiological method, deserves on that account a closer consideration. The determination of the transformation products in various media inoculated with bog bacteria should possess an exactness and a reliability that should make it suitable for the solution not only of agricultural but of ecological problems as well. It is hardly necessary to go into further detail. It is only too clear that the need for new investigations is pressing, and that especially new points of view and new lines of research are imperatively required.

From the point of view of limiting environmental factors, structural differences do not seem to play much part in enabling plants to exist in bog conditions, for the bog xerophytes, as CLEMENTS contends, are dry-land plants, which have retained the distinguishing marks of the original habitat. They are characterized by their ability to adjust themselves to changed conditions without undergoing a corresponding change of structure. Indeed, drought resistance too often results from exceptional vigor and power of endurance rather than from persistence of ancestral or new variant characters. Injury and death result only when the condition surpasses the maximal limit. The

inspection of even a few experiments with these distinctions in mind is likely to convince anyone at all familiar with such observations, that the best functioning plants, rather than the general average, represent the proper test of the possibilities of the species under the given conditions. Adjustment to local conditions is in these cases a more striking phenomenon than acclimatization, and it is to be looked upon as quite apart from structural divergence. While conclusive experiments may not as yet be admissible, the data at hand make it highly probable that the resistant forms among the hydrophytes and mesophytes in the mixed bog formations arise through gradual rather than sudden development, and that functional variation is the essential criterion. One of the results of introducing agricultural varieties into bog conditions is the comparative lack of uniformity in type, variants departing in both directions from the normal, and different varieties responding in different ways to the same conditions. Planting a species in a new place is in the nature of a test of the stability of its characters under the new conditions. Some changes result in a new place-function, but many of the changes of character that occur do not serve as responses, but result in wide individual differences and deterioration from loss or from disturbance of functional adjustments to previous established conditions. To inquire, therefore, into the nature of the adjustments of disturbed characters is to gain an indication, not so much of the many forms that the plant is able to assume, as especially the possibilities of the restoration of a balanced expression of functions which thus allow the most advantageous changes again to be established. An adapted and resistant form, if segregated through continued selection, may thus prove the basis of valuable data in the study of plasticity and fixity of organs or of type. It may prove doubtful whether the substratum solution is of a nature to bring out the greatest variation in form and in resistance. Experiments will have to be performed on a larger number of plants of widely different relationship before definite conclusions can be reached. Much economic value would attach to an extension of these experiments by using a salt or a mixture of more than one salt to ascertain a physiologically balanced solution for other kinds of plants. As has been stated above, there is an indication that the toxicity of the habitat is not the same for all agricultural plants and forest trees,

and that, hence, from the point of view of the economic importance of these results, it may be found that a certain species, that is, that one crop, is better adapted than another to withstand the effects of this type of soil. Certainly an opportunity exists through such studies for an increase in the utility of these soils, and for a better understanding as to the character and the nature of functional responses of plants adapted to them.

Leaving out of consideration for the present studies of plants which deal with functional variability and with selection, we may speak with much more confidence of the fact which environmental factors exert upon diversity of function. This effect can be much more readily studied and shows itself much more conspicuously. In fact, a knowledge of drought resistance in plants within a physiologically arid habitat will be gained mainly by securing data as to the relation of plants to environmental factors. It is quite generally agreed that transpiration is one of the most important criteria in physiological ecology. Transpiration data are more conveniently obtained, if not more serviceable in this respect, than data on respiration differences or on green and dry weight of plants. Aside from the physiological responses of roots and leaves, the most significant physical conditions which more than others directly affect transpiration in plants, are the supply of available water to the roots, and the evaporation power of the air. On account of this distinctive feature, the problem separates itself into two phases, each of which must be considered by itself. The one relates to the influence of the varying edaphic conditions, the other to the effects of climate. The two are quite different, and each is of relatively great importance. Taken by themselves, neither should be unduly drawn upon in the interpretation of drought or other resistance. The futility of the point of view which disregards all but one or two influences is seen clearly enough in the laboratory. But the inadequacy of such proposals is still more apparent when we attempt field work.

A consideration of the physiological causes underlying the conditions stated has led to the conclusion that for the local study extensive investigations in the heat requirements of bog plants and the location of their minimum temperature below which growth ceases (MERRIAM, also SWINGLE) do not have the importance which must

be ascribed to temperature as a limiting factor for regions widely separated geographically. Except for the fact that growth in local bogs begins earlier in spring and continues later in autumn than in bogs of more northern regions, the data at hand on the differences between air and soil temperatures and on the total temperature exposure of plants covering a period of three years, show that low substratum temperatures, i.e., winter temperatures below 0° C. at the one-foot level (30^{cm}) and below +3° C. at the 5-foot level (1.5^{m}), do not exist here, and hence play no part in bog structure or bog development. The more detailed account of this and other phases of the study appears elsewhere. The values of both heat-conductivity and heat-diffusion are in general lower in peat than in soils or in water, and hence prevent a rapid loss of temperature in the peat strata below the surface of vegetation. A persistence of the winter cold and ice through the summer months has not been observed in this region. Contrary, therefore, to the accepted interpretation, it is found that the imperfect decomposition of bog plants is not dependent upon supposedly low temperatures. One can readily understand that if any influence does exist the process of peat formation and preservation must be due to the presence of bog toxins and their antiseptic properties.

LIVINGSTON has shown that the evaporating power of the air furnishes an extremely valuable criterion for the differentiation between great centers of plant distribution, and also for the differentiation of certain habitats and the succession of plant societies. In connection with the problem of the transpiration values of bog plants, atmometer readings were obtained from the two stations on the bog island during the past year. A third instrument was established on the campus near the University Observatory, in an open place with an exposure to wind and sunlight similar to that in the central station on the bog. The readings of the instrument on the campus were taken three times daily, in connection with the climatological observations called for by the U. S. Weather Bureau Service. The data collected on the evaporating power of the air in the bog habitat, if compared with the evaporation rate on the campus of the University, show that the evaporating power of the air in bogs is not an important limiting factor in controlling bog vegetation, or determin-

ing the character of it. It is on the University campus that the greatest rate of evaporation was constantly recorded. The differences in the rate of evaporation between the campus and the central station on the bog island varied from 16.6 per cent to 50.4 per cent—a relative rate of evaporation at times more than twice the rate observed in the bog habitat. This relation seemed wholly unexpected and very surprising at first, for the campus area on the basis of its vegetation cannot be considered xerophytic. The general indications, therefore, point to the conclusion that at Buckeye Lake the evaporation from the water surface and from the vegetation produces a vapor blanket. The action of this invisible vapor blanket influences to a great extent the rate of transpiration of bog plants, for it conserves the moisture that would otherwise be lost by evaporation. The character of the flora and the structural conditions of bog plants, therefore, cannot be ascribed to a greater evaporating power of the air, nor to alterations in temperature conditions as has been stated so frequently. The weight of evidence from this and other investigations is again decidedly in favor of the conclusion that the real determining factor in the bog habitat is the ratio of the possible rate of water absorption to the rate of transpiration, and that the toxicity of the bog habitat has a primary rôle in bringing about bog conditions.

The extent of incontrovertible facts is still scanty. Only of late has the question of the physiological aridity of a bog habitat been removed from opinion, scientific and unscientific, to the field of experiment; and further experiments will be necessary to set clear the many complicated questions still connected with this inquiry.

Botanical Laboratory
Ohio State University

PHYSIOLOGICAL ASPECTS OF FERTILIZATION AND HYBRIDIZATION IN FERNS[1]

W. D. Hoyt

(WITH TWELVE FIGURES)

The present investigation was undertaken as the first part of a study of the physiology of cell fusions. The first thing to be done in such a study is to determine what cells will fuse under normal conditions; the next step is to discover, in cases where fusions do not normally occur, the point in the events occurring in normal fusions where the process has stopped, and, so far as possible, the conditions causing the cessation of this process; the third step will be to discover conditions permitting the fusion of cells which will not fuse under normal conditions.

The results obtained in the present study will be considered under three heads: (1) under the heading *Hybridization* the evidence bearing on the occurrence of hybrids in ferns will be considered, and the results of many attempts to induce experimentally hybridization between many species and several genera will be given; (2) under the heading *Fertilization* it has been possible to show, in some of the cases where fusions have not been obtained, at what point in the progress of normal fertilization the process has stopped, and to indicate some of the factors determining this cessation; (3) under the heading *Movements and reactions of sperms* will be given the observations made to determine whether the reactions of fern sperms are of the same kind as those described for protozoa or whether they are of a different kind.

A. Hybridization

1. HISTORICAL AND CRITICAL

A fern hybrid was described as early as 1837 (**29**), when the nature of the spores and the prothallus were still unknown and the ideas concerning reproduction among pteridophytes were still shadowy.

[1] Contributions from the Botanical Laboratory of the Johns Hopkins University, No. 13.

In this case two species, *Gymnogramme chrysophylla* and *G. calomelanos*, were growing near each other; spores of the former, when planted, produced ferns which were supposed to show characters approaching those of the other species. This case is described here because it shows a tendency which has persisted among students of ferns to the present day, that of describing as hybrids plants which are supposed to show intermediate characters, without sufficient evidence as to the real origin of these plants.

All later studies of fern hybrids fall into three groups. The first includes the numerous cases where plants have been found in the field or greenhouse differing in some respects from the characters considered as typical for one species, and supposedly approaching in these respects the characters considered as typical for another species. The second group includes those cases where spores, prothalli, or portions of prothalli of two or more species have been planted together in the hope that cross-fertilization might occur, and plants arising from these cultures showing supposedly intermediate characters have been described as hybrids. The third group is composed of a single case where sperms of a known species were presented to prothalli of another species bearing only archegonia, and the attempt was made to determine whether development of the embryo occurred or not.

The first group is represented by the works of LUERSSEN (**28**), DRUERY (**10, 11**), Miss SLOSSON (**45**), HAHNE (**15**), DOWELL (**8**), BENEDICT (**1, 2**), and others. FOCKE (**14**) lists the supposed hybrids described before 1881, and concludes that, except in the genus Gymnogramme, all are doubtful. It is not evident why FOCKE excepts Gymnogramme in this conclusion.

The reasons for regarding the kind of evidence considered by these authors as sufficient to prove the hybrid nature of the supposed crosses are given by BENEDICT (**2**) in a recent paper. These reasons are (1) the intermediate character of the supposed hybrid, (2) its sterility, abnormality, and greater vigor as compared with the supposed parents, (3) its occurrence being found only occasionally and then usually with the supposed parents. BENEDICT states, however, that "irregular plants, which are to be explained as due to ecological conditions or as sports rather than as hybrids, are not unusual. The immediate

presence of the parent species is not a necessity, since fern plants are often long-lived, and the greater vigor of the crosses might render them resistant to conditions sufficiently adverse to destroy the parent species." While many of the plants studied by BENEDICT and others may be hybrids, it would be extremely difficult to distinguish, in such cases, between "sports" and "hybrids," especially when the supposed parents are not present, and the conclusions founded on such evidence must always have a large element of uncertainty. Ferns, like other plants, are known to vary widely under different conditions, and certain tendencies toward variation are recognized by students of this group (**31**, p. 272).

The uncertainty of conclusions founded on a study of the sporophyte is emphasized by the work of SADEBECK (**40, 41**). This author, sowing spores of *Asplenium adulterinum* and of *A. Serpentini* on a serpentine-free substratum for successive generations, obtained respectively *Asplenium viride* and *A. Adiantum nigrum*, thus showing that forms previously described as species were but serpentine varieties. The first transformation to the non-serpentine form of the species occurred in the former case in the fourth, in the latter case in the fifth generation, the serpentine form being maintained for several generations on a serpentine-free substratum. Even cultures, therefore, unless continued for many generations under different conditions, may fail to show the real nature of a supposed species.

In discussions concerning hybridity, much weight is given to the point that in the characters in which the specimens differ from the "type of the species," they approach those of some other species. This may be due to hybridization as supposed, or it may be due to some other cause. The serpentine form of *Asplenium viride* ("*A. adulterinum*") assumes, in many respects, an intermediate form between the "typical" *A. viride* and *A. trichomanes*, showing that an ecological variety may assume a form approaching that of another species.

If species were clearly recognizable units with distinct limits, and if these units were changeable only by hybridization, we should be justified in describing intermediate forms as hybrids. But, considering the cases mentioned above and the variation that is known to

occur in all groups, this kind of evidence seems entirely inadequate to prove the hybrid nature of any individual. While, if all the three lines of evidence given above concurred in indicating any single form as a hybrid, they would furnish some reason for so regarding it, but they would not furnish conclusive proof of its hybridity. This proof could be obtained only by producing experimentally the form in question from a cross between cells of known parentage.

The second class of evidence is given in the works of DRUERY (**9**), FARMER (**12**), LOWE (**27**), Miss SLOSSON (**46, 47**), HANS (**16, 17**), and others. The supposed hybrids described by these authors were obtained by planting together spores, prothalli, or parts of prothalli of different species and trusting to chance to obtain a cross. Some of the resulting offspring were adjudged hybrids because of their apparently intermediate character between their supposed parents. This evidence, while more trustworthy than that considered above, is not beyond question. LOWE obtained such various results from his sowings of mixed spores that he was led to propose the theory of "multiple parentage" to account for all the differences in the offspring. MAXON (**30**) lists the three authentic cases of hybridization between species as the *Phyllitis scolopendrium* (*Scolopendrium vulgare*)×*Ceterarch officinarum* described by DRUERY (**10, 11**), the *Polypodium vulgare elegantissimum*×*Phlebodium aureum* described by FARMER (**12**), and the *Polystichum aculeatum*×*Polystichum angulare* described by LOWE (**27**) and DRUERY (**11**).

DAVENPORT (**6**) considers the last two cases uncertain and expresses no opinion about the first case. He concludes (p. 7): "I do not see how we can ever obtain absolute proof of hybridity in nature, because it is manifestly impossible for us to observe the process through which it is brought about; nor do I see how we can obtain positive proof through mixed sowing, as it is equally impossible for us to determine from which species the germinating spores come. It seems to me that in all such cases there will always be an element of uncertainty that will compel us to rely altogether upon inductive reasoning for our conclusions, which are not always likely to be safe."

The best work in this line is that of Miss SLOSSON (**46, 47**). By planting together portions of the prothalli of the supposed parents, she obtained offspring resembling the plants previously described

as hybrids, *Dryopteris cristata*×*D. marginalis* and *Asplenium platyneuron*×*Camptosorus rhizophyllus* (the well-known *Asplenium ebenoides*). These results are almost convincing, but do not place the subject beyond question. Cutting does not, in every case, separate the antheridia and archegonia; then, too, the prothalli formed branches and produced new sexual organs, so that, in the case of *Asplenium platyneuron* and *Camptosorus rhizophyllus*, many plants of each species appeared together with a few of the supposed hybrids. These experiments, therefore, are open to the objection raised by Miss SLOSSON (**46**) herself against the method of planting spores or whole prothalli together. They prove that the offspring come from one of the species planted, but do not prove whether this is by hybridization or some other method of variation. So far as the author is aware, no experiments have been made to prove that *Asplenium platyneuron* may not at times give rise to *Asplenium ebenoides*. Specimens of *Asplenium pinnatifidum* observed by COPELAND (**5**) approached, in their variable characters, *Camptosorus rhizophyllus* and bore a striking resemblance to *Asplenium ebenoides*, although no Camptosorus was found in the immediate neighborhood.

DRUERY (**9**) observed variation in plants of *Crytomium falcatum* and *Lastrea pseudo-mas cristata* produced apogamously. In another experiment he obtained from spores of a single plant of *Athyrium plumosum* offspring showing almost as many differences in the leaves as were found by LOWE in his "plants with multiple parentage."

In a recent article LEAVITT (**23**) has described the origin, through vegetative variation, of the Pierson and Whitman ferns, and has described the particular type of variation called "homoeosis" in ten species of wild ferns distributed among six genera.

It is clear that other means than hybridization may produce variations among ferns, and that some of these variations may give characters approaching those which are supposed to belong to other species. This being the case, we must regard as unproved all cases of supposed hybridization founded on structural characters of the sporophyte.

In the third class of evidence mentioned comes the work of VOEGLER (**51**). This is the one case of experimental studies on hybridization in ferns where the experimenter attempted to work with known units;

it is, therefore, the only case where the results are trustworthy. VOEGLER presented the sperms of several species to the archegonia of other species and attempted to determine, by observing the living sperms after their entrance into the archegonium, whether they fused with the egg or not. The following combinations were tried: *Dicksonia antarctica*♀ × *Nephrolepis davalloides*♂; *Ceratopteris thalictroides*♀ or *Nephrolepis davalloides*♀ × *Dicksonia antarctica*♂, or *Blechnum occidentale*♂, or *Gymnogramme Laucheana*♂. In more than 100 cases of entrance, no fusion of the sperm with the egg was observed. VOEGLER also presented the sperm of several species to the prothalli of other species bearing only archegonia, and, having replanted these, examined them after one or two days to determine by the appearance of the archegonia whether development of the egg had begun or not. In all cases negative results were obtained; the archegonia had turned brown.

There are obvious objections to the methods used: (1) there were not sufficient precautions to insure the absence of sperms on the archegonial prothalli; (2) observations on the surface can scarcely give certain conclusions about the fusion of sperm with the egg; (3) reliance is placed on the appearance of the archegonia after one or two days to determine whether development of the egg has occurred or not. Since in the two species where the time has been noted (SHAW **42**, CONARD **4**) the first division of the egg does not occur until after seven days, and since in this investigation the same time has been found to hold for other species, the method used does not seem beyond question.

In this work of VOEGLER'S, the most reliable study of hybridization in ferns, all the results are negative. The author concludes that while hybridization is not thereby disproved, this must seldom occur and then only between certain species.

2. METHODS

Since the value of all work of this sort depends on knowing with certainty the origin of the sexual cells brought together in the archegonium, it will be well to describe in some detail the methods used for insuring this in the present investigation.

The prothalli used were in every case raised from spores. The

determination of every species used was made or verified by Mr. W. R. MAXON. Antheridial prothalli were placed in water on a slide, and prothalli bearing only archegonia of the species necessary to make the desired cross were added to these; the entrance of sperms into the archegonia was then followed under the microscope. After being left on the slide for one to four hours, the prothalli were either planted on soil in separate pots or were fixed and studied in section to see whether fusion of sperm and egg had occurred. About half of the prothalli in which experimental fertilization was attempted were planted and the other half sectioned.

In this method there are four places where an error is especially likely to occur: (1) in gathering the spores of several species in one day, these may become mixed; (2) in planting the spores, or after these are sown, spores of other species may fall into the pot and contaminate the culture; (3) fertilization may have taken place before the prothalli are removed from the pot for the experiment; (4) the female prothalli may bear antheridia.

All these dangers were especially guarded against. In collecting spores, mature fronds were chosen, and being shaken as little as possible were carefully wrapped in several thicknesses of paper. The fruiting portions of the fronds were touched as little as possible, and the hands were washed or carefully wiped with a damp cloth between each collection.

The spores were sown on soil which was sterilized by heating to 150° C. for six to twenty-four hours. All sowings were made in rooms in which no other ferns were ever kept. When more than one sowing was made on any day, these were made in widely separated parts of the rooms, and after each sowing all the surrounding furniture and walls were wiped with a wet cloth. Before and after each sowing the hands were carefully washed and all instruments used were sterilized by boiling. The pots were kept in the laboratory where no other ferns were grown, or in the greenhouse. In this room of the greenhouse there were three fruiting specimens of ferns, but these were on the opposite side of the room. The cultures were covered with glass. In spite of these precautions, a small amount of moss appeared in several of the pots, and fungi occasionally made their entrance. It is not believed that the cultures were contaminated by

fern spores, however, because more than 100 young sporophytes from the different pots were obtained and raised to a distinguishable age, and all were of the species which was planted in the pot from which they came.

In order to prevent the occurrence of fertilizations in the pots, all cultures were watered very sparingly and only from below—by pouring water into the saucers. The soil and air were kept just moist enough to keep the prothalli in good condition. The soil was not allowed to become wet and no water was allowed to collect on the glass. In spite of these precautions, many fertilizations took place in some of the pots. Except in one case noted, however, no culture was used for archegonia unless it was free from sporelings for at least a week before and two weeks after the experiment.

In most cases the prothalli used for archegonia were examined under the microscope before being placed in water, and any portions of the prothalli bearing antheridia were cut off, and all adherent male prothalli were removed. In some cases the prothalli were not examined dry, but were placed directly in water. Antheridia or male prothalli, if present, were removed before escape of the sperms had occurred. All such cases were recorded, and no female prothallus was used for crossing if there was any reason to believe that sperms of its own species were present. By these means it was possible to be fairly sure that no antheridia of the species to be used as female were present; sometimes, however, antheridia were overlooked. In cases where the prothalli were fixed, therefore, the entire prothallus was sectioned and every section was examined for antheridia. Over 150 prothalli were thus sectioned and examined, and among these, antheridia were found in only two cases where they had not been detected by the previous examination. This method, therefore, seems to be a satisfactory one.

From the fact that the above methods were carrried out faithfully, the conclusion seems warranted that, in this work, we are dealing with sperms and eggs of known parentage.

3. RESULTS

Abundant entrance of sperms has occurred into archegonia of the same species and of different species. When the sperm and egg were

of the same species, many fusions were obtained; but in not a single case among the combinations tried was fusion obtained between an egg and a sperm of different species, in spite of the fact that sperms were in a few cases observed in contact with the egg.

The results of this portion of the investigation are shown in the following tables. In the first table are given the results of the experiments where the prothalli were sectioned and examined for fusions; in the second table are given the results of the experiments where the prothalli were replanted on soil in the effort to obtain sporelings.

In the first table only those cases are included in which sperms were observed, in the sections, within the venters; in several other cases sperms were found in the necks of the archegonia, but not in contact with the egg. Not every entrance means the possibility of fusion, since entrance often occurs in archegonia whose eggs are bad and even in empty archegonia. It is impossible, however, to tell with certainty from sections whether an egg is good or bad. In order to eliminate the error of personal judgment, entrances have been counted in all cases where the venter contained an egg, regardless of whether this egg was good or bad. It was very evident in many

TABLE OF RESULTS OBTAINED FROM SECTIONS

♀	♂	Number of prothalli	Number of entrances	Number of fusions
Dryopteris noveboracensis..	D. noveboracensis	6	10	7
D. noveboracensis.....	D. Thelypteris	8	67	0
D. Thelypteris.....	D. noveboracensis	1	1	0
D. marginalis.....	D. marginalis	5	0	0
D. Thelypteris.....	D. Thelypteris	1	0	0
D. marginalis.....	D. Thelypteris	4	1	0
D. Thelypteris.....	D. marginalis	5	1	0
D. marginalis.....	D. noveboracensis	16	5	0
D. marginalis.....	Pteris multifida	8	9	0
Pteris multifida*.....	P. multifida	41	78	26
Athyrium Filix-foemina.....	P. multifida	14	15	0
A. Filix-foemina.....	A. Filix-foemina	6	9	4
A. Filix-foemina.....	Asplenium montanum	5	6	0
A. Filix-foemina.....	As. platyneuron	11	17	0
A. Filix-foemina.....	As. platyneuron and A. Filix-foemina	2	9	1
As. platyneuron.....	A. Filix-foemina and As. platyneuron	2	1	0
As. platyneuron.....	Camp. rhizophyllus	2	1	0
Camptosorus rhizophyllus ..	As. platyneuron	4	4	0
As. montanum.....	As. Bradleyi	28	2	0

* *Pteris multifida* Poir., CHRISTENSEN in Index Filicum 602. 1906.

TABLE OF RESULTS OBTAINED FROM PROTHALLI REPLANTED ON SOIL

♀	♂	Number of prothalli	Number of entrances	Number of sporelings
D. marginalis	D. marginalis	6	0	0
D. Thelypteris	D. Thelypteris	5	0	0
D. marginalis	D. Thelypteris	23	0	0
D. marginalis	D. noveboracensis	1	3	0
D. marginalis	Pteris multifida	10	0	0
Pteris multifida	P. multifida	24	24	5
A. Filix-foemina	P. multifida	3	10	0
A. Filix-foemina	A. Filix-foemina	43	36	6
A. Filix-foemina	As. montanum	3	0	0
A. Filix-foemina	As. platyneuron	8	10	0
As. platyneuron	As. platyneuron	40	?	6
As. platyneuron	A. Filix-foemina and As. platyneuron	2	6	2*
As. platyneuron	Camp. rhizophyllus and As. platyneuron	1	1	0
Camp. rhizophyllus	As. platyneuron	12	6?	0

*As. platyneuron.

of the entrances counted that the egg was bad, but the egg is no more likely to be bad in the cases of the entrance of a foreign sperm than in the cases of the entrance of its own sperm. It is believed, therefore, that the proportion of entrances in which fusions have been obtained may be compared in the two cases.

In the second table, the number of entrances obtained can only be approximated, since on the one hand not every entrance is observed, and on the other hand an observed entrance does not necessarily mean that the sperms have reached the venter.

These results for most of the combinations tried are not conclusive, but taken all together they constitute a considerable mass of evidence against the occurrence of hybridization in these ferns. Entrance of sperms of its own species into 97 archegonia on 59 prothalli, as shown in sections, gave 37 fusions; while the entrance of sperms of other species into 129 archegonia on 111 prothalli failed to give a single fusion.

The results obtained from replanting the prothalli on soil confirm those obtained from sections; all point against the occurrence of hybrids among these ferns.

The most noteworthy cases observed in sections are the following: Sixty-seven entrances of the sperms of *Dryopteris Thelypteris* into the

archegonia of *D. noveboracensis* gave no fusions; while ten entrances of its own sperms into the archegonia of *D. noveboracensis* gave seven fusions.

It should be stated that most of these 67 eggs looked bad. Some, however, seemed good and appeared to be in the stage in which fusion would have been expected to occur if sperms of its own species had been present. Young archegonia in various stages were present on the prothalli sectioned, and sporelings appeared in this culture of *Dryopteris noveboracensis* before, during, and after the time that the experiments were made, so that it seems very improbable that the eggs were bad at the time of entrance in all the 67 archegonia in which entrance was obtained. In view of the supposed hybrids described among Dryopteris species by DOWELL (**8**) and BENEDICT (**1, 2**), this result is of considerable interest.

Using *Athyrium Filix-foemina* as the female parent, the entrance of the sperms of *Pteris multifida* into fifteen archegonia, and of the sperms of *Asplenium montanum* into the six archegonia, and of the sperms of *Asplenium platyneuron* into seventeen archegonia gave no fusions, while the entrance of its own sperms into nine archegonia gave four fusions.

In one attempted cross of *Asplenium platyneuron* sperms with *Athyrium Filix-foemina* eggs, antheridia of *A. Filix-foemina* also were present. In this case nine entrances were obtained and one fusion resulted. In the attempted reciprocal cross of *Athyrium Filix-foemina* sperms with *Asplenium platyneuron* eggs, sperms of *Asplenium platyneuron* were present; six entrances and two sporelings of *Asplenium platyneuron* were obtained. Evidently when sperms of two species are present, fusion may still take place between the egg and sperm of its own species; the presence of foreign sperms does not prevent this fusion.

As these experiments were made at the same time as the attempted crosses between the eggs of *Athyrium Filix-foemina* and the sperms of *Asplenium platyneuron*, they show that the failure to obtain fusions in that case was not due to a bad condition of the eggs or sperms.

The results given above do not disprove the existence of hybrids or the possibility of obtaining them. Owing to lack of material and to various accidents during the investigation, experiments were not made

on a sufficient number of closely related species, and the results in most cases are not sufficiently conclusive to settle the question. But these experiments do show that not every combination of egg and sperm will yield a hybrid. Such a result is to be expected, but in view of the supposed cross between *Camptosorus rhizophyllus* and *Asplenium platyneuron*, it cannot be assumed without proof. A priori, we should expect hybrids to occur in some cases, but the fact remains that in the two investigations on the subject where known elements have been used, only negative results have been obtained.

B. Fertilization

It is well known that fusion will occur between the sexual cells of different species in some cases and will not occur in others, but few attempts have been made to determine in the cases where fusion does not occur the point at which the processes stop and the conditions causing this cessation, or to induce fusion in cases where it does not occur under normal conditions.

In ferns the process of normal fertilization has been described by STRASBURGER (**48**), SHAW (**42**), VOEGLER (**51**), THOM (**50**), MOTTIER (**34**), CONARD (**4**), and YAMANOUCHI (**53**). PFEFFER (**36**) and VOEGLER (**51**) obtained entrance of the sperms of one species into the venters of other species in every combination tried. VOEGLER attempted to follow in more than 100 living prothalli the movements of sperms which had entered the venters of other species. In every case, although the sperms came into contact with the eggs, they were unable to enter, and finally ceased their movements, or after several attempts to enter the egg left the archegonium. Presumably the phenomena observed were the same as those described below.

In the present investigation attraction of the sperms and their entrance into the venter has been obtained in every combination used, and in several cases the living sperms have been observed and their movements followed within the venters of the same species and of different species. In most cases the sperms were seen as an actively moving mass lying against the egg on the side turned toward the archegonial neck, the so-called receptive spot. The egg looked round and turgid. The individual sperms, when distinguished, had their anterior ends apparently in contact with the egg and were lashing

about vigorously and revolving on their axes. Sometimes no locomotion was observed, the entire mass of sperms appearing to remain at the receptive spot, but often some of the sperms were observed to change their places and occasionally to leave the venter. In one case five sperms of *Athyrium Filix-foemina* were observed within an archegonium of the same species. All of these swam about within the venter and were observed to leave and reenter the venter several times. This prothallus when replanted on soil gave no embryo; the egg, therefore, was probably bad. In every case when sperms were observed within the venter and fusion resulted, the mass of sperms was observed to remain in contact with the egg. Perhaps a good egg attracts the sperms, while a bad egg does not.

Since in the cases mentioned the sperms were observed to bore against the receptive spot of the egg of another species as vigorously as they bored against the egg of their own species, it seems that the failure of such sperms to penetrate the egg is not due to any failure of the sperm to perform its part in the process. The failure of such sperms to enter the egg seems to be due either to some hindrance on the part of the egg or to some interaction of egg and sperm. There is little evidence to help us to decide between these two possibilities.

VOEGLER observed that, after the entrance of one sperm, a second sperm, although boring against the egg, was unable to enter. This would appear to be due to some change occurring within the egg, perhaps comparable to the formation of a membrane in the fertilized animal egg.

FARMER (**13**) suggests that the entrance of more than one sperm into the egg is prevented by the occurrence of chemical changes in the cytoplasm of the egg immediately upon fertilization. He believes that these chemical changes produce substances which are injurious to the sperms, and presents some evidence for such an occurrence in the alga Halidrys. The entrance of a second sperm is not, however, always prevented. MOTTIER (**34**) has figured two sperms within one egg of *Onoclea Struthiopteris*, and WOODBURN (**52**) has described two remarkable cases of polyspermy in the same species; in one of these, seven sperms were observed within the egg nucleus. Whether similar phenomena may occur in other species is not known.

MORGAN (**32, 33**) has shown that in the ascidian Ciona, the sperms

are practically incapable of fertilizing the eggs of the same individual and of certain other individuals, and has suggested that this is due to substances present in the egg furnishing a sort of "immunity" against these sperms.

NEWMAN (**35**) was able, by treating the eggs of Fundulus with solutions of salts that precipitate colloids and increase the surface tension, to destroy the permeability of the egg membrane to the sperm, and then, by treating such eggs with solutions of salts that dissolve precipitated colloids and decrease surface tension, to restore the permeability of the membrane. The results of MORGAN on Ciona, according to this author, are capable of being explained by the assumption that different eggs and sperms have different relative surface tensions. NEWMAN states that "these experiments and those of LOEB and MORGAN seem to indicate that the relative surface tension of the egg and of the sperm is one important factor governing fertilization."

MOENKHAUS, however, in cross-fertilizing fishes that in many cases belong to widely separated orders, often found the percentage of eggs impregnated to be as great as in the check experiments where the sperm and eggs were of the same species. When eggs of *Fundulus heteroclitus* were subjected to a mixture of equal parts of sperms of the same species and of sperms of *Menidia notata* or *M. gracilis*, there were often more fertilizations by the foreign sperms than by the sperms of Fundulus. The foreign sperms were prepotent. MOENKHAUS concluded that, among the forms used in the experiments, there is no specific adaptation such as chemical affinity, surface tension, etc., but that the prepotency of the foreign sperm is due to its being more active and hence a swifter fertilizer.

TENNENT (**49**) was unable to obtain crosses between different genera of echinoderms when the sperms were added to the eggs immediately after their removal from the ovary, but obtained fusion in every combination tried when the eggs had been allowed to stand for several hours. The time after removal from the ovary when fertilization with foreign sperms can be obtained is definite for any particular combination and is different in different combinations; it appears to depend on the species of sperm as well as on the egg employed. The eggs, after standing and becoming capable of fertilization by

foreign sperms, are still able to be fertilized by their own sperms. Apparently changes take place progressively in the egg after its removal from the ovary, so that at different times it is capable of being fertilized by different species of foreign sperms, but these changes do not prevent the entrance of its own sperms. In a paper soon to be published, TENNENT considers the effect of variation in the concentration of OH ions in the sea water in which the eggs stand before and during fertilization, and suggests that the variation in alkalinity brought about by artificial means in the experiments may imitate a natural seasonal variation in the concentration of the OH ions in ordinary sea water.

In crossing the eggs of the starfish *Asterias ochracea* with the sperms of the sea urchins *Stronglyocentrotus purpuratus* and *S. franciscanus*, LOEB (**26**) found that in sea water the sperms would fuse with the eggs of their own species, but not with those of the starfish; while in sea water made slightly alkaline, the sperms would unite with the eggs of the starfish, but not with those of their own species.

The failure of fern sperms to fuse with eggs of their own species which are just past maturity seems to be due, in part, to the physical condition of these eggs. Eggs which are evidently good have clear, soft-looking surfaces; while eggs as they become bad have darker surfaces that resemble more and more the appearance of a dried colloid. While it may be safely assumed that chemical changes are taking place within the egg at the same time, these physical changes would alone probably be sufficient to prevent the entrance of sperms.

We have no evidence to help us to decide whether a similar explanation would account for the failure of a second sperm to enter an egg of its own species, since no constant difference between fertilized and unfertilized eggs was detected. An attempt to apply the same explanation to the lack of fusion of foreign sperms meets with still greater difficulties. A careful scrutiny failed to show that eggs of other species, which as mentioned were not entered by sperms, had thicker membranes than did the eggs of their own species. If such a structure were present, we should find eggs of other species with thinner membranes than those of their own species, and should

be able to obtain entrance of sperms into them; but no such case has been observed.

It is well known that different degrees of fusion of egg and sperm are obtained in different combinations. In some cases apparent fusion occurs, but the offspring remain sterile; in others a partial fusion occurs, but the offspring fail to develop; while in others no fusion occurs. A good discussion of the cases where after fusion the offspring remain sterile or undeveloped is given by DEVRIES (**7**). Such cases seem to be due to some interrelation of the chromatin content of the two sexual cells. The failure of egg and sperm to fuse seems to be caused by some physical or chemical interaction between the two. It remains to be seen whether there is any sharp line between these two classes of phenomena.

The possibility of fusion seems to be determined by different factors in different species of plants and animals. In some fish there is no specific adaptation; in echinoderms changes occurring after the removal of the eggs from the ovary permit the fusion of sperms of different species at different intervals of time; in some echinoderms the alkalinity of the sea water determines the possibility of the fusion of certain sperms; in some cases the relative surface tension of egg and sperm may determine the possibility of fusion; in other cases the presence of certain substances in the egg may prevent the fusion of certain sperms; and lastly double fertilization may be prevented by the formation of certain substances or by physical changes in the egg. The possibility of fusion seems, then, to be determined in some cases chiefly by physical, in others by chemical conditions.

In ferns we have no evidence of the existence of any physical barrier in the ripe egg that might prevent the entrance of foreign sperms. In the present state of knowledge, it seems probable that the failure of egg and sperm to fuse is due to some interaction between the two. It is greatly to be hoped that we shall some day be able to obtain crosses. We may then be able to obtain some evidence concerning the nature of this interaction.

C. Movements and reactions of sperms

The accounts of the movements of motile organisms may in general be considered in two groups. In the first of these, the

positive response of an organism to a stimulus is as follows: the organism, swimming about at random and coming to that portion of the medium in which the stimulant is present, has its movements directly modified by the stimulating agent, so that the direction of its axis is turned toward the region of stimulation; it then proceeds in a straight line toward the stimulus. This is the "local action theory of tropisms" discussed by JENNINGS (**18, 19**), and includes the "strophic" movements of ROTHERT (**39**) and the "topotactic" movements of PFEFFER (**38**). According to the other view, the organism, coming into the region of a stimulant to which it reacts "positively," enters this region without reacting, but upon tending to leave the region, it reacts by turning back; the "positive" response is thus obtained by a series of "negative" reactions whenever the organism tends to go in a direction leading away from the source of stimulation. This is the reaction described by JENNINGS (**18, 19**) for many protozoa and bacteria, and includes the "apobatic" movements of ROTHERT, and the "phobotactic" movements of PFEFFER. Similar explanations to those accounting for the positive reactions in the two views are given for negative reactions.

According to the former account, the source of stimulation is the unequal distribution of the stimulant on different parts of the organism; this acts locally on the organism, directly influencing the motor organs; and the essential part of the reaction is the orientation of the organism so that its axis is placed parallel to the direction of greatest stimulation. According to the latter account, the source of stimulation is the change of the conditions to which the organism is subjected; this change acts on the organism as a whole; and the essential nature of the reaction is that the organism shows no response in passing toward the optimum, but reacts by turning back upon tending to pass from the optimum.

The reaction of motile plant cells to chemical stimuli has been studied by PFEFFER (**36, 37**) in ferns, Marsilia, Selaginella, mosses, liverworts, Chara, bacteria, the swarmspores of Saprolegnia, and certain flagellates; by LIDFORSS (**24, 25**) in Marchantia and Equisetum; by SHIBATA (**43, 44**) in Salvinia and Isoetes; by VOEGLER (**51**) and BULLER (**3**) in ferns; and by ROTHERT (**39**), KNIEP (**22**), and JENNINGS and CROSBY (**20**) in bacteria.

PFEFFER (**38,** p. 757) suggests that, in rapidly reacting swarmcells, the positive, apparently "topotactic," response may be the result of a series of negative responses as is outlined above, but considers the reactions of many flagellates, the swarmspores of many algae, the sperms of ferns, and the swarmspores of Saprolegnia to be accomplished by tropistic orientation, "durch die typische tropistische Richtung der Körperachse" (**38,** p. 754).[2] The same view is held by LIDFORSS for the movements of the sperms of Marchantia and Equisetum. SHIBATA explained the positive reactions of Isoetes sperms in the same manner, but considered certain negative reactions shown by them to be phobotactic, believing that both kinds of reaction exist in the same organism. The same possibility is suggested by LIDFORSS for the sperms of Marchantia. VOEGLER and BULLER do not discuss the nature of the reaction. ROTHERT showed that certain bacteria react only in tending to pass from the optimum in the manner outlined above; they thus finally collect in the region of optimum stimulation.

JENNINGS (**18**) has shown that the reactions of many of the lower organisms consist of a series of random movements performed when the organism tends to pass from the optimum, with the continuance of such of these movements as take it toward the optimum. The movements leading toward the optimum may in Stentor and other organisms be performed more readily after repetition, and in some organisms the final response—toward the region of optimal stimulation—may be attained with practically no random movements, thus giving a directive reaction. The difference between this type of reaction and that favored by the holders of the other view mentioned is, that in this one it is held that the stimulant acts on the organism as a whole and that the organism responds as a whole, performing complex and coordinated movements; in the other it is held that the stimulant acts on certain parts of the organism, directly modifying the action of these parts. According to JENNINGS, the reaction obtained depends on the physiological condition of the organism, which is in turn partly determined by the past experiences of the organism.

[2] The reference to the German edition is given here because of the abridgment of the English translation at this point.

The positive response of fern sperms to the archegonium and to the salts of malic acid is too well known to need description. This is believed by most authors to be due to the directive action of the stimulating substance on the motor organs, the axis being thus turned toward the stimulant. Most workers, however, while holding this view, have noted that many sperms do not react as thus described, but may wander about the field, or pass through it indifferently, or may avoid it. Such reactions have been ascribed to individual differences.

Thus JOST (**21**, p. 542) states that "whenever they (the sperms) come into the neighborhood of an archegonium, they twist sharply round, so as to direct their anterior ends toward the mouth of the archegonium; they thus rapidly approach it, enter its neck, and fuse with the ovum in the interior." In describing experiments with malic acid, JOST states (**21**, p. 542): "It may be clearly seen that the sperms curve around sharply the moment they come within the sphere of influence of the malic acid diffusing out of the tube, and place their long axes parallel with the course of the diffusion current. Without any acceleration of their movements they then steer their way toward the more concentrated solution straight for the mouth of the tube. Since the sperms distribute themselves equally in a homogeneous solution of malic acid, just as they do in water, we are bound to regard the unequal distribution of the acid as the directive stimulus."

The question before us is: Are the movements of fern sperms best explained as being due to the local action of different concentrations of a stimulant on different parts of the body, thus producing movements leading to an orientation of the axis; or are they to be regarded as the result of the action of the stimulant on the organism as a whole, the resulting movements being such as to take the organism toward the region of optimal concentration?

In the present investigation a few observations were made on the reactions of sperms to malic acid in capillary tubes, but most of the results on positive responses were obtained from the reaction of the sperms to the substance extruded from the archegonia of either the same or of different species. In such cases the prothalli bearing the antheridia and the archegonia were rinsed and placed in water

on the slide under a cover glass, and the movements of the sperms followed. For other experiments the antheridial prothalli, after being rinsed, were placed in a few drops of water; and the sperms, after their escape, were transferred to the slide with a pipette. Since the sperms are too small to be studied with a binocular microscope, most of the observations were made with an ordinary microscope, using a Zeiss 8^{mm} objective. The results given are based on the observation of thousands of sperms; they will, of course, not apply to every sperm whose movements were followed, but will apply only as a kind of average, stating in general the manner of reaction observed.

Most of the observations were made on the sperms of *Pteris multifida*,[3] but these results were verified with the sperm of every species studied. The structure of the sperms is that of a flat, spirally coiled band bearing numerous cilia, especially near their anterior ends. Some of them are coiled to the left, others to the right; some of them appear to revolve clockwise, others in a counter-clockwise direction, and some of them seem at times to change their direction of rotation.

In water the sperms swim by rapid movements of their cilia, at the same time rotating on their axes and swinging their anterior ends through small circles. The result of this is that they progress in narrow spirals; their course in water approaches a straight line, but has occasional turnings. When the sperms are placed in quince seed jelly or a solution of India ink, however, their movements are strikingly different from those shown in water. They now swing their anterior ends through large circles, so that they proceed along wide spirals; they no longer pursue a course approaching a straight line, but turn here and there again and again. In thick ink they pass through spirals larger than those made in thin ink. *Fig. 1* shows the differences in the spiral courses of sperms in thick ink, in thin ink, and in water. When they change the direction of movement, they accomplish this by swinging their anterior ends through large circles, at the same time revolving on their axes, and then going forward in a

[3] The material of *Pteris multifida* used in this investigation was obtained from Jamaica, B.W.I., during a visit made by the writer to that island, aided by a grant from the Bache Fund. Acknowledgment is here made to the trustees of that fund for this assistance.

new direction. The result of this is that they experience the conditions on all sides before they continue their course in a new direction. In going in a new direction they may turn up, down, or to one side. Apparently the direction toward which they turn is that toward which the anterior end of their spiral points, but their small size made it impossible to determine this with certainty.

The movements shown in the negative and positive reactions are of the same kind as those described above. If a crystal of some salt, such as potassium nitrate, sodium chloride, etc., be placed at the edge of a drop containing sperms, a solution repellent to the sperms diffuses into the water. Sperms approaching this solution show various movements. Some of them swing their anterior ends through large circles, at the same time revolving on their axes, and then go forward as described above; such sperms often turn almost immediately from the stimulant and leave the injurious region. Most sperms, however, swing their anterior ends through smaller circles and make small turns. Such a turn may carry them into the solution, alongside of it, or away from it. If it carries them away, they usually continue this course; but if it carries them in or alongside, they turn again and again until they are headed away from the source of stimulation, and then continue forward. These sperms, therefore, usually wander here and there about the field before being headed away from the stimulant. *Fig. 2* illustrates the course pursued by such sperms.

In a study of the positive responses of the sperms to the archegonium, it is immediately seen that the reactions are varied. While some sperms seem at first sight to turn directly and enter the archegonium immediately (*figs. 3* and *4*), others enter only after wandering about the field, occasionally going past the archegonial mouth and then turning back (*figs. 5* and *6*), while others pass indifferently (*fig. 7*), or change their courses without entering (*figs. 8–10*), or turn and leave the region (*figs. 11* and *12*). The sperms swimming in water move with great speed, but upon reaching the region in front of an open archegonium, they are slowed up by the extruded slime. A close examination of those sperms which seem to enter by turning directly to the archegonium shows that some of these slow up suddenly and swing their anterior ends through large circles, at the same time

revolving on their axes, as previously described; they then go forward. This movement usually results in their turning toward the arche-

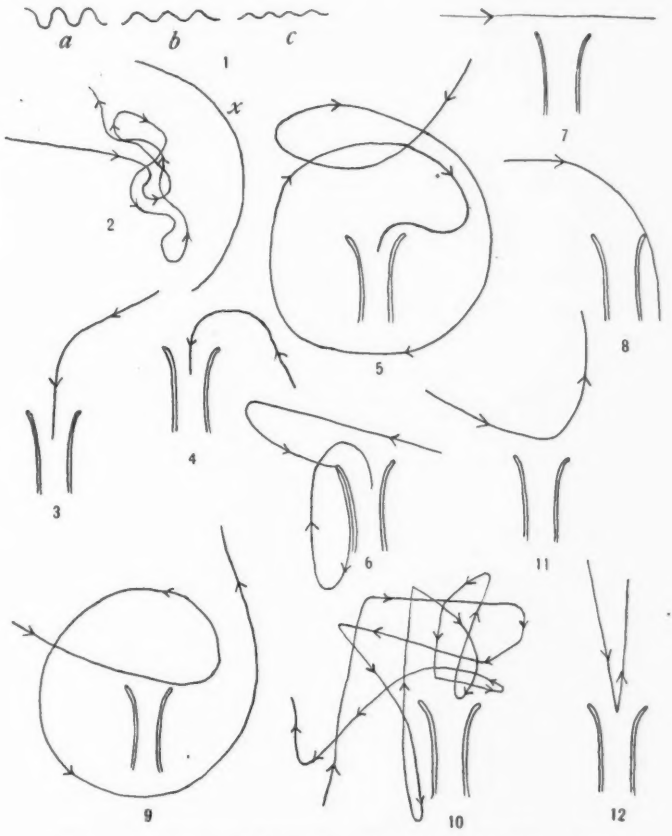


FIG. 1.—Spiral courses of sperms: *a*, thick ink; *b*, thin ink; *c*, water.
FIG. 2.—The position of the salt crystal is indicated by *x*.
FIGS. 3–12.—Various responses of sperms.

gonium, but not directly toward it; and their subsequent orientation and entrance into the archegonium is attained by a series of gradual swingings toward the archegonial mouth as they go forward. Most sperms, however, react less immediately and attain their orientation

with respect to the archegonium, not by one large swing, but by a series of small swingings of their anterior ends and rotation on their axes as they go forward, thus reaching the archegonium by a number of successive changes in their spiral course. This reaction is characteristic for those sperms whose movements were sufficiently slow to be followed exactly. A few sperms were observed pursuing a markedly spiral course to the archegonium, and turning back whenever their movements tended to carry them away from it; and in all these cases where the movement was sufficiently slow to be followed, the sperms attained their orientation by a series of gradual swingings of their axes toward or away from the archegonium.

Occasionally, the sperms are observed to collect in a certain region, either in front of an archegonium or at some spot on a prothallus, and to swim about in this field. These react by turning back when they reach the boundaries of a fairly definite region, and only occasionally does one leave the group. Sperms entering this region do not react, but upon passing through and tending to leave it on the other side they react by turning back (*fig.* 10). These reactions are strictly comparable with those described by JENNINGS for Paramecium, and with the "apobatic" movements described by ROTHERT for certain bacteria.

It has been mentioned that some sperms will approach an archegonium without entering. Many of those which have been attracted and have gathered at the mouth of a full neck will suddenly leave the field of attraction; others which have entered and have passed down the neck will suddenly emerge at great speed, although others at the same time are being attracted and are entering. Since others are attracted it does not seem probable that the attractive substance has become repellent. We must believe, rather, that these sperms have become physiologically changed, so that they now react negatively to a substance to which they previously reacted positively.

Such emerging sperms are long and drawn out to almost a straight line; they shoot out violently from the archegonium, often forcing their way through the crowd of sperms collected at the mouth. They sometimes emerge backward, but usually come out with their anterior end foremost. They are sometimes seen to come from a great distance down the neck, apparently from the venter, and may begin

to emerge within two minutes after the entrance has occurred. Sometimes they remain almost motionless in front of the archegonium and die after a few minutes; but often they recover and swim away. The movements of such sperms are strikingly different from normal movements. They move away quickly in almost a straight line, sometimes swerving slightly from their straight course, sometimes, upon striking an obstacle, turning slightly to one side and passing this; or they may move about irregularly, with irregular rotation on their axes. They move more in one plane than do the normal sperms, and do not show the marked swinging of their anterior ends that is characteristic of the normal movements. Often such sperms gradually regain their spiral form and their normal spiral movements, and finally swim off in a fairly normal manner. A few sperms which had left the archegonium were observed to go to the boundary of the attractive region, and then turn and reenter the archegonium.

The movements of the sperms under other conditions are exceedingly complex and varied. When sperms are placed in a very thick quince seed jelly, or when they enter far into a strong salt solution, they constantly swing their anterior ends about in large circles, at the same time rotating on their axes; they turn here and there, constantly changing their direction of movement, and frequently changing their direction of rotation. Thus one sperm in thick jelly was observed moving slowly forward in short jerks, by half-revolutions first in one direction and then in the other. Such movements are continued as long as the sperms are alive.

Sperms which have become attached to some solid body by their cilia or their vesicles perform almost every conceivable variety of movement which is possible for them under these conditions. They frequently whirl their unattached ends round and round in one direction, and then suddenly reverse this, making several or many turns in the other direction. After continuing this whirling for some time, they often swing from side to side several times and then continue whirling round and round as before. They give occasional violent jerks and swings, sometimes almost breaking themselves in two, and continue their complex movements until they die or free themselves.

Often sperms approach an open archegonium without entering,

but remain at the mouth, uncoiling and coiling again and lashing about with their posterior ends. Since other sperms enter at the same time, it is difficult to explain these movements as being due to an injurious effect of the substance extruded from the archegonium.

On several occasions a peculiar reaction of the sperms was observed. This resembled the darting movements often seen in a swarm of gnats and will be described as the "darting reaction." Sperms exhibiting these movements collected in a fairly definite region, and showed almost constant motion, keeping one end still and swinging the other end about, or darting about in the field, here and there, with constant changes in their direction of movement. They move in every direction, sometimes approaching the archegonium, sometimes passing it, sometimes going away from it. Occasionally one proceeds in a fairly direct spiral to the boundary of the region, then turns directly back, approaches the archegonium, and resumes its darting movements. Such sperms usually do not enter the archegonium directly, but may wander in and out of the mouth. Some, however, remain in the archegonium, so that an accumulation of sperms finally occurs within the archegonial neck. A few of the sperms leave this field, but most of them remain in it until they die. Other sperms upon entering the field show the darting reactions immediately, and usually react by turning back whenever they approach the boundary of the region. This collection usually occurred in front of an open archegonium, but sometimes at other places, presumably where some of the cells of the prothallus were injured. The movements described were shown especially in the reactions of sperms of the same species and of other species toward the archegonia of *Athyrium Filix-foemina*, but were not shown in every case of entrance in this species, and were shown in a few cases of entrance in other species.

It now remains for us to consider which of the views stated above will best explain the movements and reactions described. We will first sum up the observed facts. It has been shown that any interference with the movements of sperms, whether by jelly, thick ink, a salt solution, or attachment to a solid particle, produces a series of complex movements; the sperms swing their anterior ends through large circles, at the same time rotating on their axes, and then go

forward. If these movements free them from the hindrance, they resume their normal movements; but if the hindrance continues, they perform other complex movements, finally performing almost every conceivable variety of movement that is possible for them, and they continue these movements until they die or free themselves from the obstacle.

A few of the sperms showing the positive and negative reactions appear to turn directly toward or away from the stimulant. These swing their anterior ends through large circles, rotating on their axes, and then go forward in the new direction. Such movements often do not result, however, in the complete orientation of the sperms with respect to the stimulus; the final orientation is then attained by further successive swingings of their axes as they go forward. Most sperms attain their orientation by such small swingings of their axes toward or away from the stimulant as they go forward.

Occasionally sperms collect in a definite region by passing into this without reacting, and then turning back whenever they tend to leave it.

Finally, it has been shown that different sperms react differently to the same stimulus at the same time, and that the same sperm may react differently to the same stimulus at different times. In some cases these differences in reaction are due to different physiological conditions induced by different past experiences.

All these facts indicate that the reactions of fern sperms are due to the action of stimulants on the organism as a whole, and not to the direct action of a stimulant on local parts of the organism. The sperms possess a certain mechanism of reaction, and this mechanism is called into play by a change of conditions or by an interference with the normal movements. This mechanism is different in different sperms, and may be altered by different external conditions.

The strongest evidence for the direct orientation of the axes of the sperms by the stimulant is furnished by those cases in which the sperms respond by large swingings of their anterior ends and then go forward, turning fairly directly toward or away from the source of stimulation. Such cases do not prove, however, the "local action" theory of the reaction. These sperms may be regarded as having a mechanism of response by which they swing about until they are

headed toward or away from the region of greater stimulation. They experience the conditions on all sides before continuing in their new direction, just as do those sperms which attain their orientation by many small spiral swingings in different directions. Furthermore, their new direction does not always take them directly toward or away from the source of stimulation, and their subsequent orientation is then attained by successive changes of their course as described for other sperms. Their reactions thus seem to be of the same kind as those described for other sperms. Sperms which react in this manner form a small part of those observed, and the reactions of other sperms do not accord with the "local action theory." It has been shown that some sperms collect in a region by strictly "phobotactic" responses. If we hold that the sperms described above react by "topotactic" movements, therefore, we must suppose that in both the positive and negative responses different sperms of the same species respond to the same stimulus by reactions of different natures. This is hard to believe. It seems, rather, that the sperms mentioned have their mechanisms of response called into play by the effect of the stimulant on the organism as a whole, and not by the action of the stimulant on local parts of the organism.

The reactions of fern sperms thus seem to be of the same kind as those described for protozoa.

Summary

1. Entrance of sperms into archegonia was obtained in every combination of species tried.

2. When the egg and sperm were of the same species, entrance into 97 archegonia, as shown by sections, resulted in 37 fusions; but when the egg and sperm were of different species, entrance into 129 archegonia failed to give a single fusion. A similar result was obtained by replanting prothalli on soil after entrance had occurred.

3. The results obtained, while not disproving the existence of fern hybrids, indicate that conclusions based on the structure of the sporophyte should not be accepted without additional experimental evidence, and show that not every combination of egg and sperm can result in a fusion.

4. Sperms of one species were observed within the archegonia of another species boring against the egg. The failure of such sperms

to enter the egg does not seem to be due to any failure of the sperm to perform its part in the process.

5. The failure of such sperms to enter the egg seems to be due either to some hindrance interposed by the egg, or to the interaction of egg and sperm. Since no means of hindrance on the part of the egg could be detected, it seems probable that this failure of the sperms to enter the eggs is due to an interaction of the egg and sperm. Such an interaction may be physical or chemical, or more probably both physical and chemical.

6. The movements of fern sperms are complex and varied. The reactions of the sperms depend on their physiological state, and this depends in part on the past experiences of the sperms.

7. Sperms have a mechanism of response which is called into action by a change of conditions or by an interference with the normal movements. In such cases they perform a series of complex movements and continue these until they die or free themselves from the stimulus.

8. Orientation of the sperms in both the positive and negative reactions is usually attained by a series of gradual swingings of their anterior ends accompanied by a rotation on their axes, and not by a sudden turning toward or away from the stimulant.

9. The observed movements and reactions seem due to the effect of the stimulant on the organism as a whole, and not to the action of different concentrations of the stimulant on local parts of the organism.

10. The reactions of fern sperms thus seem to be of the same kind as those described for protozoa.

The author is gratefully indebted to Professor D. S. JOHNSON and Professor H. S. JENNINGS, of the Johns Hopkins University, for much helpful advice and assistance; to Mr. W. R. MAXON, U.S. National Herbarium, for the determination of the species used in this investigation, and for helpful suggestions; to Professor W. J. MOENKHAUS, Indiana University, and to Dr. D. H. TENNENT, Bryn Mawr College, for permission to refer to their unpublished results; to Dr. CAMPBELL WATERS, Washington, D.C., and to Mr. W. N. CLUTE, Joliet, Ill., for assistance in completing the bibliography; and to Mr.

AMEDEE HANS, Stamford, Conn., for information concerning his results in the attempted crossing of ferns.

THE JOHNS HOPKINS UNIVERSITY
BALTIMORE

LITERATURE CITED

1. BENEDICT, R. C., Some fern hybrids. Science N.S. **27**:542–543. 1908.
2. ———, New hybrids in Dryopteris. Bull. Torr. Bot. Club **36**:41–49. 1909.
3. BULLER, A. H. R., Contributions to our knowledge of the physiology of the spermatozoa of ferns. Annals of Botany **14**:543–582. 1900.
4. CONARD, H. S., The structure and life history of the hay-scented fern. Carnegie Inst. Washington. 1908.
5. COPELAND, E. B., Two fern monstrosities. BOT. GAZETTE **34**:142–144 *figs.* 5. 1902.
6. DAVENPORT, G. E., Abnormal forms and hybridity in ferns. Papers presented at Boston Meeting of Linnaean Fern Chapter. pp. 1–11. 1899.
7. DEVRIES, HUGO, Fertilization and hybridization. English translation by C. STUART GAGER. The Monist **19**:514–555. 1909.
8. DOWELL, PHILIP, New ferns described as hybrids in the genus Dryopteris. Bull. Torr. Bot. Club **35**:135–140. 1908.
9. DRUERY, CH. T., Apogamic ferns. Gard. Chronicle **18**:211–212, 305–306. 1895.
10. ———, A bigeneric fern hybrid. Gard. Chronicle **18**:365. 1895.
11. ———, Fern crossing and hybridizing. Jour. Roy. Hort. Soc. London **24**: 288–297. *figs.* 5. 1900.
12. FARMER, J. B., On the structure of a fern hybrid. Annals of Botany **11**: 533–544. *pls. 23, 24.* 1897.
13. ———, On the structural constituents of the nucleus, and their relation to the organization of the individual. Proc. Roy. Soc. London Bot. **79**:446–464. 1907.
14. FOCKE, W. O., Die Pflanzen-Mischlinge. Berlin. 1880.
15. HAHNE, H., Ueber Farnhybriden. Allgem. Bot. Zeitschr. **10**:102–106. 1904.
16. HANS, AMEDEE, About abnormal ferns. Horticulture **3**:611–613. *figs.* 4. 1906.
17. ———, *Polystichum acrostichoides*×*angulare*. Fern Bull. **16**:14, 15. 1908.
18. JENNINGS, H. S., Behavior of the lower organisms. New York. 1906.
19. ———, The interpretation of the behavior of the lower organisms. Science N.S. **27**:698–710. 1908.
20. ———, AND CROSBY, J. H., The manner in which bacteria react to stimuli, especially chemical stimuli. Amer. Jour. Physiol. **6**:31–37. 1901.
21. JOST, J., Lectures on plant physiology. English translation by R. J. H. GIBSON. Oxford. 1907.

22. KNEIP, HANS, Untersuchungen über die Chemotaxis von Bakterien. Jahrb. Wiss. Bot. **43**:213–270. 1906.
23. LEAVITT, R. G., A vegetative mutant and the principle of homoeosis in plants. BOT. GAZETTE **47**:30–67. *figs. 19.* 1909.
24. LIDFORSS, B., Ueber die Reizbewegungen der Marchantia-Spermatozoiden. Jahrb. Wiss. Bot. **41**:65–87. 1904.
25. ———, Ueber die Chemotaxis der Equisetum-Spermatozoiden. Ber. Deutsch. Bot. Gesell. **23**:314–316. 1905.
26. LOEB, J., The fertilization of the egg of the sea-urchin by the sperm of the starfish. Univ. Cal. Pub. Physiology **1**:39–53. 1903.
27. LOWE, E. J., Fern growing. New York. 1898.
28. LUERSSEN, CH., Die Farnpflanzen. RABENHORST's Krypt. Flora. 1889.
29. MARTEMS, M., Notice sur un cas d'hybridité dans les fougères. Bull. Acad. Roy. Bruxelles **4**:47–50. 1837.
30. MAXON, W. R., Notes on the validity of *Asplenium ebenoides* as a species. BOT. GAZETTE **30**:410–415. 1900.
31. ———, A study of certain Mexican and Guatemalan species of Polypodium. Contr. U.S. Nat. Herb. **8**:271–276. *pls. 61, 62.* 1903.
32. MORGAN, T. H., Self-fertilization induced by artificial means. Jour. Exp. Zool. **1**:135–178. 1904.
33. ———, Some further experiments on self-fertilization in Ciona. Biol. Bull. **8**:313–330. 1905.
34. MOTTIER, D. M., Fecundation in plants. Carnegie Inst. Washington. 1904.
35. NEWMAN, H. H., On some factors governing the permeability of the egg membrane by the sperm. Biol. Bull. **9**:378–387. 1905.
36. PFEFFER, W., Locomotorische Richtungsbewegungen durch chemische Reize. Unters. Bot. Inst. Tübingen **1**:363–482. 1884.
37. ———, Ueber chemotaktische Bewegungen von Bacterien, Flagellaten, und Volvocineen. Unters. Bot. Inst. Tübingen **2**:582–662. 1888.
38. ———, Pflanzenphysiologie 2^2:1904.
39. ROTHERT, W., Beobachtungen und Betrachtungen über tactische Reizerscheinungen. Flora **88**:371–421. 1901.
40. SADEBECK, R., Ueber *Asplenium adulterinum*. Verhandl. Bot. Vereins Prov. Brandenburg **13**:1872; reference in SCHIMPER's Plant geography. English translation. Oxford. 1903.
41. ———, Ueber die generationsweisse fortgesetzen Aussaaten und Culturen der Serpentinformen der Farngattung Asplenium. Ber. Sitz. Gesell. Bot. Hamburg. Heft. 3. 1887; reference in SCHIMPER's Plant geography. 1903.
42. SHAW, W. R., The fertilization of Onoclea. Annals of Botany **12**:261–285. *pl. 19.* 1898.
43. SHIBATA, K., Studien ueber die Chemotaxis von Isoetes-Spermatozoiden. Jahrb. Wiss. Bot. **41**:561–610. 1905.
44. ———, Studien ueber die Chemotaxis der Salvinia-Spermatozoiden. Bot. Mag. Tokyo **19**:39–49. 1905.

45. SLOSSON, MARGARET, *Dryopteris cristata×marginalis*. Plant World **2**:4–7. *pl. 1.* 1898.

46. ———, Experiments in hybridizing ferns. Papers presented at New York Meeting of Linnaean Fern Soc. pp. 19–25. 1900.

47. ———, The origin of *Asplenium ebenoides*. Bull. Torr. Bot. Club **29**:487–495. *figs. 7.* 1902.

48. STRASBURGER, ED., Die Befruchtung bei den Farnkräutern. Jahrb. Wiss. Bot. **7**:390–408. *pls. 25, 26.* 1870.

49. TENNENT, D. H., The chromosomes in cross-fertilized echinoid eggs. Biol. Bull. **15**:127–134. *pl. 1.* 1907.

50. THOM, C., The process of fertilization in Aspidium and Adiantum. Trans. Acad. Sci. St. Louis **9**:285–314. *pls. 36–38.* 1899.

51. VOEGLER, C., Beiträge zur Kenntniss der Reizerscheinungen. Bot. Zeit. **49**:641 ff. 1891.

52. WOODBURN, W. L., A remarkable case of polyspermy in ferns. BOT. GAZETTE **44**:227. *fig. 1.* 1907.

53. YAMANOUCHI, S., Spermatogenesis, oogenesis, and fertilization in Nephrodium. BOT. GAZETTE **45**:145–175. *pls. 6–8.* 1908.

VARIATION AND CORRELATION IN RAYS AND DISK FLORETS OF ASTER FASTIGIATUS[1]

H. NAKANO

(WITH FOUR FIGURES)

Through various researches on the variation of Compositae, Umbelliferae, etc., LUDWIG (**1, 2, 3, 5, 6, 7, 10**) came to the conclusion that multimodal curves prevail in plants, and that the mode always falls on some member of the Fibonacci series, or often on some member of SCHIMPER-BRAUN'S accessory series, that is, on the so-called "Nebenzahlen." When a mode comes on a number other than that in the principal and accessory series, he considers this as a *Scheingipfel*, in which two curves with modes on two adjacent classes are combined.

On the other hand, in the variation of the rays of *Chrysanthemum Leucanthemum*, LUCAS (**8**) obtained the principal mode on 22, a result which is an exception to LUDWIG'S rule, but which seemed to find confirmation later in the researches of SHULL (**12**) and TOWER (**13**). The curves of these two authors, however, were altogether multimodal and obtained from comparatively scanty material. For example, SHULL doubled the classes on account of the sparseness of his material, so that the apparent multimodal curves changed to monomodal ones. It may not be useless, therefore, to examine the applicability of LUDWIG'S rule in the variation of the rays and disk florets of *Aster fastigiatus* Fisch. and Mey., using a tolerably large number of variates.

The true significance of two different results in LUCAS'S calculation was not clearly known until TOWER'S discussion (**13**) appeared. According to him, the number of rays of *Chrysanthemum Leucanthemum* decreases continuously during the flowering season (27.87–21.38 between July 5 and July 30), and LUCAS'S curve and his own were not influenced by the difference of the place-modes.

A little before the publication of TOWER'S work, SHULL'S admirable paper (**12**) on the variation and correlation in the bracts, rays, and

[1] Preliminary note.

disk florets of *Aster Shortii*, *A. novae-angliae*, *A. puniceus*, and *A. prenanthoides* appeared. In this he recognized clearly the decrease of the number of the bracts, rays, and disk florets of *Aster prenanthoides* in the successive collections.[2]

The cause of the seasonal change of flowers in the multicipital Compositae is probably partly due to a change in nutrition, as HAACKE (**4**) and SHULL (**12**) have suggested; but an environmental condition seems to influence this decrease also. Recently Dr. KORIBA (**16**) of this laboratory found favorable material in the single-headed *Arnica unalaschensis*, which he collected on Mt. Hakkôda (Aomori), because the mean number of the rays changed exclusively from environmental changes. According to him, the mean increased from 14.389 to 15.741 between July 29 and August 5, for this was the season of thawing snow, and hence a favorable condition; then it decreased continuously to 15.081 on August 30.

During the summer of 1909 my attention was called to large patches of *Aster jastigiatus* on the banks of the Tone River, near Kokoku, Simosa Province, about thirty miles east of Tokyo. The number of rays and of disk florets was rather small, so that I was induced to calculate the coefficient of correlation between them. The present preliminary note seeks to verify LUDWIG'S rule in the variation of these rays and disk florets, and to find the seasonal change in rays and the range of correlation between rays and disk florets.

The material was taken from three patches scattered through a grassy field (about 100 meters square) along the river bank. The environmental condition of these three patches was almost the same, and seems not to affect in any degree the variation of the flowers; therefore, only the difference of time seems to account for the differences in the three collections. The first collection was made in the eastern part of the field, the second in the western, and the third in the middle. In collecting specimens care must be taken, for random collection seems to give a little larger value than picking all flowers from every individual (LUCAS **14**); and the latter method, adopted in this investigation, may probably avoid personal error. All flowers that were injured by insects or that were extremely old or young were rejected.

[2] From September 27 to October 8 the mean number of rays decreased from 30.769 to 26.335.

As shown later, the class range in an individual is very restricted, and falls perhaps on one part of the curve of the racial variation. For this reason a more reliable result can be obtained by taking into account the flowers from an equal number of individuals, rather than an equal number of flowers from an unequal number of individuals. Hence I have counted in all cases all the flowers in ninety plants. The calculation has been made wholly according to DAVENPORT (**15**), and the results obtained are as follows:

Variations of rays

The first collection was made August 7, 1909, when the flowers had first begun to bloom, and the following data were obtained:

Number	Range	Mode	A	σ	C
1392	10–31	17	17.921 ±0.051	2.831 ±0.032	15.799

The curve (*fig. 1*) of this variation gives the positive skewness (PEARSON **9**, p. 408) 0.325, the mean value being very near 18. On the mode 17 fall 17.10 per cent and on 18 fall 16.38 per cent of all variates.

The second collection was made August 11, with the following results:

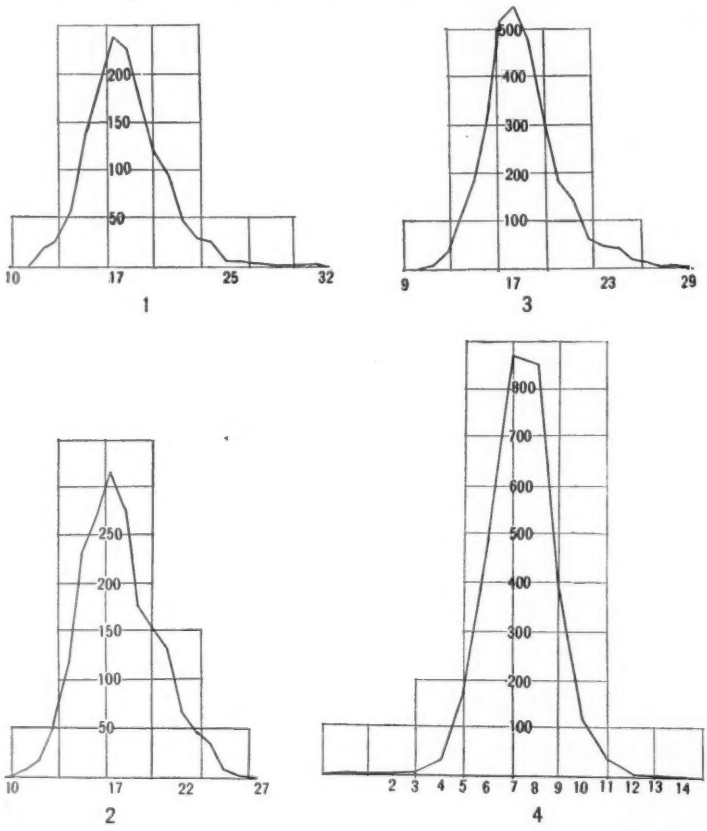
Number	Range	Mode	A	σ	C
1904	10–26	17	17.606 ±0.041	2.618 ±0.029	14.872

The percentage of variates occurring on mode 17 became a little less (16.55 per cent). This is due to the fact that the ordinates of the two classes 18 and 16 come nearly to the same height. The positive skewness became 0.231; the curve lessened the degree of asymmetry on account of the diminution in number of right-hand abscissae (*fig. 2*).

The third collection was made August 19, with the following results:

Number	Range	Mode	A	σ	C
2959	10–28	17	17.339 ±0.031	2.521 ±0.022	14.542

In this collection the range became somewhat wider than in the second, but all other elements decreased continuously. The mode came near to class 16, and the point of inflection of the curve came



FIGS. 1–4

nearer to the origin than in the two foregoing collections. The variates falling on the mode 17 increased to 18.28 per cent, and the positive skewness became only 0.134 (*fig. 3*).

On comparing the above three collections, it can be seen easily that the constants of variation decrease continuously from the begin-

ning to the end of the flowering season. The mean value of the rays decreased from 17.921 to 17.339 during twelve days, but no change of the mode was observed, the mode always holding its position on 17, which does not belong to the Fibonacci series. However, the oscillation of the mode between 18 and 16 may be observed from the first and third collections.

Variation of disk florets

This material was taken from the same flowers used in the third collection, and the constants calculated are as follows:

Number	Range	Mode	A	σ	C
2959	6–13	7–8	7.401 ±0.017	1.329 ±0.012	17.963

LUDWIG'S rule did not hold in this case, since the mode falls on 7 rather than on 8, although 7 is a member of SCHIMPER-BRAUN'S accessory series.

Individual correlation

I call briefly the correlation between the organs of the same individual the "individual correlation." PEARSON says (**9**, p. 392): "We have seen that the racial variation is greater than the individual variation, that capsules on the same poppy plant are more alike to each other than they are to the capsules of a second plant, or the leaves of one beech tree to each other than to those of a second beech tree. The resemblance of the like organs of the same individual is a special case of correlation, and we now want a quantitative measure of such correlation." I have tried, therefore, to find the value of the individual correlation, if such exists in my collections, and to throw some light upon this problem. For this purpose, I calculated the number (R) of classes and the difference (D) of the minimum and maximum classes in each individual variation, using the three collections mentioned above.

In the racial variation the class range was 10–31, that is, there occurred 21 different classes (one deficient in 11); while in the individual variation, as the above result (R) shows, only 2–10 different classes (rarely more than 6) occur, and curiously enough these classes happen to be almost continuous, as D clearly shows. From these

facts we know that all the classes occurring in any individual, fall most probably on one part (perhaps on one side) of the curve in the racial variation, and that the classes in the individual variation are almost continuous.

1. NINETY PLANTS COLLECTED AUGUST 7

R	2	3	4	5	6	7	8	9	10	Total	*D*	2	3	4	5	6	7	8	10	11	Total
f	1	4	13	24	27	14	5	1	1	90	*f*	1	11	18	22	15	16	4	2	1	90

From this point of view, we should take into account the flowers in an equal number of individuals, if we wish to compare two collections in two different conditions, and not an equal number of flowers, because in the latter case we would probably obtain a curve that declines to one side or the other. For example, when in the second collection of rays 1431 heads from 70 plants were taken into account, the mean was 17.777; and when in the third collection 1366 heads from 49 plants were taken, the mean was 17.694; these are due undoubtedly to the smaller number of individuals that were in the first collection.

I have examined also whether the shifting of *R* and *D* occurs with the change of the flowering season.

2. NINETY PLANTS COLLECTED AUGUST 11

R	3	4	5	6	7	8	9	Total	*D*	2	3	4	5	6	7	8	9	Total
f	3	7	25	28	19	5	3	90	*f*	3	4	20	24	18	11	7	3	90

3. NINETY PLANTS COLLECTED AUGUST 19

R	3	4	5	6	7	8	9	10	Total	*D*	2	3	4	5	6	7	8	9	10	11	13	Total
f	1	2	9	23	30	14	7	4	90	*f*	1	2	5	22	29	11	12	5	1	1	1	90

In the second collection only a slight change occurred, while in the third collection the number of different classes in each individual increased to 7 (the most probable one), and accordingly the number of *D* shifted from 5 to 6. These increases result from the addition of classes of lower value, to which the seasonal change of rays is due.

Correlation between the number of rays and of disk florets

The material of the third collection was used in this calculation. A glance at the correlation surface in the accompanying table shows

that correlation certainly exists in some degree. From the data of this table, I computed by PEARSON's method the coefficient of correlation between the rays and disk florets of *Aster fastigiatus*. The result of my calculation is $r=0.3219\pm0.0111$. Since the coefficient of correlation always lies between 0 and 1, my result shows a significant correlation, though not in a high degree. Generally the correlation between the rays and disk florets appears not very large (SHULL **12**), so that SHULL found this coefficient only $0.574+-0.353$ in *Aster prenanthoides;* while he obtained in the correlation of its rays and bracts 0.8559–0.7986. As to the change of the coefficient of correlation in the flowering season, which WELDON (**11**) and SHULL (**12**) discovered in another species, a future investigation of *Aster fastigiatus* is necessary.

CORRELATION SURFACE

RAYS	DISK FLORETS														TOTALS	MEAN OF DISK FLORETS
	0	1	2	3	4	5	6	7	8	9	10	11	12	13		
10	1	..	..	..	..	..	..	..	..	..	..	..	..	..	1	0
11	..	..	..	..	..	1	2	..	1	..	..	..	..	..	4	6.250
12	..	..	..	2	6	9	7	5	3	..	..	..	..	..	32	5.500
13	..	..	..	1	4	15	44	21	14	3	1	..	..	..	103	6.342
14	..	..	..	..	3	26	43	62	39	10	..	..	..	..	183	6.754
15	..	..	..	..	5	37	52	119	77	19	7	1	..	..	317	6.997
16	..	..	..	1	4	30	109	153	130	66	14	4	..	..	511	7.225
17	..	..	..	..	1	23	72	185	162	76	19	3	..	..	541	7.484
18	..	1	..	..	2	16	58	120	168	83	21	1	1	..	471	7.645
19	..	..	1	..	..	7	53	72	93	52	20	5	..	..	303	7.677
20	..	..	..	..	..	3	25	55	57	27	7	3	..	..	177	7.633
21	..	..	..	..	..	2	15	37	46	24	8	3	..	..	135	7.822
22	..	..	..	..	..	2	2	18	17	10	5	4	..	1	59	8.153
23	..	..	..	..	..	..	9	10	18	8	..	3	..	..	48	7.711
24	..	..	..	..	..	1	1	11	15	3	7	4	..	1	43	8.419
25	..	..	..	..	..	..	..	4	7	2	3	1	..	..	17	8.412
26	..	..	.1	..	..	..	1	..	2	..	4	2	1	..	10	9.600
27	..	..	..	..	..	..	..	..	..	1	1	..	..	..	2	9.500
28	..	..	..	..	..	..	..	..	1	..	..	..	1	..	2	10.000
Totals ...	1	1	1	4	25	172	493	872	850	384	117	34	3	2	2959	

My thanks are due to Professor MIYOSHI for his valuable suggestions.

Summary

1. In the variation of the number of the rays and disk florets of *Aster fastigiatus* Fisch. and Mey., the curve is always monomodal, and its mode does not belong to the Fibonacci series.

2. The seasonal change in the number of rays is clearly observable in this species.

3. All the classes in individual variation have a tendency to fluctuate on one side or other of the curve of the racial variation, and appear to be almost continuous.

4. The number of rays and that of disk florets change correlatively to each other in a tolerably marked degree.

BOTANICAL INSTITUTE, COLLEGE OF SCIENCE
IMPERIAL UNIVERSITY, TOKYO, JAPAN

LITERATURE CITED

1. LUDWIG, Ueber Variationskurven und Variationsflächen der Pflanzen. Bot. Centralbl. **64**: 1895.
2. ———, Eine fünfgipfelige Variationskurve. Ber. Deutsch. Bot. Gesell. **14**: 1896.
3. ———, Weiteres über Fibonacci-Curven. Bot. Centralbl. **68**: 1896.
4. HAACKE, Entwickelungsmechanische Untersuchungen. Biol. Centralbl. **16**: 1896.
5. LUDWIG, Das Gesetz der Variabilität der Zahl der Zungenblüten von *Chrysanthemum Leucanthemum*. Bot. Centralbl. **71**: 1897.
6. ———, Nachträgliche Bemerkungen über die Multipla der Fibonacci-Zahlen und die Coexistenz kleiner Bewegungen bei der Variation der Pflanzen. Bot. Centralbl. **71**: 1897.
7. ———, Ueber Variationscurven. Bot. Centralbl. **75**: 105. 1898.
8. LUCAS, Variation in the number of ray flowers in the white daisy. Amer. Nat. **32**: 509. 1898.
9. PEARSON, The grammar of science. 1900.
10. LUDWIG, Variationsstatistische Probleme und Materialien. Biomet. **1**: 11. 1901.
11. WELDON, Change in organic correlation of *Ficaria ranunculoides* during the flowering season. Biomet. **1**: 125. 1901.
12. SHULL, A quantitative study of variation in the bracts, rays, and disk florets of *Aster Shortii*, *A. novae-angliae*, *A. puniceus*, and *A. prenanthoides*. Amer. Nat. **36**: 1902.
13. TOWER, Variation in the ray flowers of *Chrysanthemum Leucanthemum*, etc. Biomet. **1**: 309. 1902.
14. LUCAS, Variation in the ray flowers of the common cone flowers (*Rudbeckia hirta*). Amer. Nat. **38**: 1902.
15. DAVENPORT, Statistical methods with special reference to biological variation. 1904.
16. KORIBA, Variation in the ray flowers of some Compositae. Bot. Mag. Tokyo **22**: nos. 256–258. 1908.

CURRENT LITERATURE

BOOK REVIEWS

A history of botany

In considering a work professedly containing selections from the contributors to botany,[1] it is important to discover the reasons for the choice. Professor GREENE states that it is not his purpose to write a history of botany, or to treat in chronological succession of those who have upbuilt the science. As the introduction discusses a "philosophy of botanical history," the author's point of view may be looked for there.

There have been famous treatises on the philosophy of history, but there has been so little agreement that some are skeptical as to its reality. The author illustrates this from botanical writers, and concludes that "everyone may be permitted to have his own philosophy." He proceeds to state his own in the following words: "Upon the historian of botany it seems to devolve that he shall have some forecast of what botany in its perfection as a science shall be like; for in practice he sits in judgment on each epoch and decides whether as an epoch its tendency was more to the advancement of the science or to its retardation; from which kind of procedure it becomes certain that some ideal of perfection is in his mind." What the ideal is in this history may be inferred from the closing paragraph of the introduction: "I am unwilling to conclude this introduction without repeating it, that the essence and substance of botany proper are organography and the logical deductions that we draw from organography. They may not be said to be the whole of the science, yet duly and comprehensively considered they will be found to come near it. The line of development of organography—organography as necessarily including terminology—is that along which a truly coherent and philosophic history of botany must needs be written." It is legitimate to ask how far the subjective is involved in this, and how it may shape the ideal of the historian. The work gives evidence throughout that the taxonomic features of botany and the related subjects receive the fullest treatment. Organography comes to its own in systematic botany. No fault can be found with this in itself, for each man should do what he is best qualified to do by training and experience. But when we deal with history as such, there is danger of confusing the advocate and the historian.

Considerable pains are taken to show that classifying is a very ancient process,

[1] GREENE, EDWARD LEE, Landmarks of botanical history. A study of certain epochs in the development of the science of botany. Part I, Prior to 1562 A.D. 8vo. pp. 329. Published by the Smithsonian Institution, Washington. 1909.

and that the roots of the science of botany are to be found before there was anything written. The author states that he has found no allusion among botanical writers to the fact of the universal existence of a crude primitive system of plant classification. The reviewer recalls that Dr. WHEEWELL, although not a "botanical writer," in his *History of the inductive sciences* develops in considerable detail the same idea, using botany and zoology as the best representatives of the classificatory sciences. The author seems to have laid emphasis on these older stages to show that due credit is not given to the older writers. This plea appears frequently in the book; one is not allowed to forget it. Such men as ADANSON, TOURNEFORT, and LINNAEUS were improvers, not creators. While granting all the truth there is in this, it seems to the reviewer that its importance has been overestimated. There has been some neglect in this respect, but the lack of definiteness and the changeableness in applying names and definitions have furnished a plausible excuse for the neglect.

Like SPRENGEL and MEYER, the author begins with the rhizotomi, or root-gatherers, who sought plants for religious, culinary, or medicinal purposes. Their experience, traditions, and written accounts were drawn upon by THEOPHRASTOS and others. The longest chapter, more than one-fourth of the volume, is given to THEOPHRASTOS, the "father of botany." It is based on a study of his *Historia plantarum*, a very full summary of its contents being given, with conclusions derived from their study. They are presented under the following heads: methods, vegetative organography, anthology, fruit and seed, anatomy, phytography, taxonomy, nomenclature, ecology, dendrology, and transmutation. I find phenology given in the case of TRAGUS, and physiology and pomology in that of VALERIUS CORDUS. These topics indicate what the author has sought or found in the works that have been studied. It is evident that he made a careful study of the *Historia plantarum*. In a recapitulation, containing seventeen items, there is a "list of facts botanical which THEOPHRASTOS saw, and in the main discovered." It is said to embrace "well-nigh all the first rudiments of what even today is universal scientific botany. It illustrates superabundantly the fact that THEOPHRASTOS, and no man of later time, is the father of the science as we now have and hold it." I find no reference, except in the biography and in two footnotes, to the other principal work of THEOPHRASTOS, the *De causis plantarum*, which in WIMMER'S edition of his works, in pure Greek text without note or comment, takes only twelve pages less than the *Historia*. It would not fit so well into the author's ideal, since it deals more with matters physiologic, ecologic, and especially economic, how plants behave, how they are to be treated in cultivation, etc. MEYER complains that botanists, in their proclivity for the *Historia*, have "hitherto neglected in an unjustifiable way" this other "not less important" work.

Another chapter deals with those Greek and Roman authors whose botanical writings have survived. The Greeks are NICANDER, who wrote in verse on poisons and their antidotes; and DIOSCORIDES and GALEN, physicians, whose works are pharmaceutical and medical chiefly. Among the Romans the most important is PLINY, much like DIOSCORIDES in his treatment of plants; VERGIL,

with his *Georgics* and *Bucolics;* and the writers on agriculture whose works are known under the title *De re rustica*. While all of these writers contributed something to botany, about all the phytography that has come to us from the ancients is to be found in the works of THEOPHRASTOS, DIOSCORIDES, and PLINY.

The remainder of the volume is taken up with those who have been called by the German writers "the fathers of German botany," or "the botanical reformers of the 16th century." Beginning with BRUNFELS and FUCHS, it is shown that to them is due an improved iconography, but not any reform in phytography. Their drawings from nature were substituted for the wretched and incorrect figures of the old herbals, but the accompanying descriptions were translations or compilations from the ancient authors. It was assumed that the plants of the Mediterranean region, which the old books described, grew in the fields and forests of Germany. Some changes were made by BRUNFELS in grouping plants, foreshadowing genera more akin to those now recognized, and real reform in phytography was made by BOCK, better known as TRAGUS. He studied plants in the field, and those not included in the older books were described in his clear and graphic style. Professor GREENE regards him as the "first father of phytography after THEOPHRASTOS." Although often paying close attention to the floral parts, he was still dominated by the idea that likeness in foliage, stem, and root, and sensible qualities like odor and taste, were better criteria of affinities than similarities in fruit and seed.

The two remaining chapters are devoted to EURICIUS CORDUS and his son VALERIUS CORDUS. EURICIUS published only one botanical work, the *Botanologicon*, but it so fully exposed the mistake of identifying the plants of Germany with those of the ancient writers that a decided advance was made in botany. To VALERIUS CORDUS the author assigns a high position, and from all that is known of his life and work he was an exceptional man. Dying at the age of twenty-nine, from exposure to the miasmatic climate of that part of Italy he was exploring in the heat of summer, it is felt that what was so well done in a life so short would have been greatly extended had his life been prolonged. In his *Geschichte der Botanik*, MEYER speaks of him as "a shining but fleeting phenomenon," and says "few have accomplished work of so many kinds and so great in so short a life." He was a lecturer on medicine, a botanical explorer, and a writer; and also a chemist and a mineralogist. TOURNEFORT speaks of him as "the first of all to excel in the description of plants"; and MEYER says that "his descriptions surpassed those of all his predecessors in precision and in the clearness with which they were brought home to perception" (*Anshaulichkeit*). Professor GREENE outlines the orderly plan of description adopted by CORDUS, and considers his special title to distinction to be that of "the inventor of the art of phytography," in doing away with the need of pictures of plants, and by showing that "every species could be so characterized in words as to be identifiable by description alone." His *Historia plantarum* was left in manuscript and was not published until several years after his death.

In these *Landmarks* Professor GREENE has made a very interesting and

important contribution to the history of botany for a period which is comparatively inaccessible to readers of English alone. One appreciates how gradually the science has evolved. Although the epoch-makers of the modern science were to come later, the roots of many of the leading principles are here disclosed. The style is clear and animated, requiring no needless effort to grasp the author's meaning. An excellent index is an additional source of satisfaction.—E. J. HILL.

Vegetation dynamics in the desert

There is no habitat where the vegetation appears more static than in the desert, but SPALDING[2] has now clearly shown that well-defined successions are to be found there as well as elsewhere, and that the "struggle for existence" involves "competition" between plant individuals as well as strenuous relations with untoward physical surroundings. The material here presented is the result of several years of intensive study on Tumamoc Hill (the location of the Desert Laboratory) and the adjoining valley, and it is concluded that the distribution of plants in that neighborhood can be accounted for by causes now in operation. The first chapter considers the plant associations and habitats. The river and irrigating ditches are relatively poor in aquatics. The river margin is fringed with an association in which cottonwoods and willows dominate, while the mesquite (*Prosopis velutina*) is the dominating species of the floodplain; an interesting phenomenon in the latter habitat is the invasion of the more xerophytic *Bigelovia Hartwegii*. Salt spots are present with characteristic halophytes, but the most pronounced of the latter (i.e., *Suaeda Moquini*) grows more luxuriantly along irrigating ditches than in salt spots. The washes, which are dry drainage channels, are characterized by the palo verde (Cercidium) and catclaw (*Acacia Greggii*), while the more xerophytic slopes are tenanted largely by the creosote bush (Larrea) and Franseria associations. The hill is characterized by Fouquieria and Parkinsonia on any exposure, while the giant cactus (*Cereus giganteus*) and *Encelia farinosa* are essentially south-slope species; *Lippia Wrightii* equally characterizes north slopes, which are also much richer in the aggregate number of species and individuals than are other slopes. An account of the lichens is given by FINK, Acarospora being the most characteristic genus; the lichens generally are unusually xerophytic in structure.

The second chapter considers the detailed distribution (with maps) of some of the more characteristic species, and the third chapter has to do with environmental and historical factors. An account of the geology of the region is presented by TOLMAN, and of the soils by LIVINGSTON. There are extensive tables depicting the soil moisture, rainfall, temperature, and evaporation. MACDOUGAL contributes an interesting chapter on the origin of desert floras; xerophytes are regarded as of recent origin, and the view, formerly current, that the desert xerophytes have arisen through adaptation is opposed, at least as a general explanation. The final chapters give a review, discussion, and summary.

[2] SPALDING, VOLNEY M. Distribution and movements of desert plants. pp. v+144. *pls. 31*. Carnegie Institution of Washington, Publication 113. 1909.

SPALDING concludes that even in deserts soils play a great part in distribution, and that physiographic factors play an efficient part as in the East. He finds that as the region is gradually base-leveled, the area of the mesquite association widens, that the palo verde-catclaw association advances similarly along the washes, and that the creosote bush advances along the slopes. The latter species is of wide range, appearing in areas as a pioneer and also remaining as the final possessor; and yet the xerophytic structures of this plant are simple as compared with the spectacular features of the giant cactus, whose range is much more restricted. The soil factors that appear most important are the soil water content, the percentage of alkali, and the soil texture (involving aeration). That not all distributional factors reside in the soil is shown by the fact that the giant cactus is chiefly an inhabitant of south slopes (attributed to the need for high temperatures), and especially by the fact that *Lippia Wrightii* is restricted to the north slope about the laboratory, while it is just as definitely restricted to the south slope in the mountains at an altitude of 1000^{m} above the laboratory. SPALDING has here given us one of the most notable of recent contributions, clearly showing that the intensive study of plant associations is quite as productive in results in deserts as in mesophytic climates.—H. C. COWLES.

Evolution

It is a matter for congratulation when the subject-matter of any great field is gathered together and systematized by a master hand. The first volume of LOTSY's lectures on the theories of descent has been reviewed in these pages,[3] and the second volume should have been noticed sooner.[4] The first chapters of the second volume consider various phases of Darwinism, noting first the factors that influenced DARWIN's investigations. The various prerequisites of Darwinism, such as variability, selective value, and the struggle for existence, are first considered. It is recognized that DARWIN did not attempt to explain variability, assuming it as given. Nor did DARWIN distinguish sharply between variations and mutations, a view that seems to the author to be borne out by KLEBS's experiments. After lectures on orthogenesis, selective value, the struggle for existence, the inheritance of deviations, there follow several lectures on the facts of paleontology and plant geography that are explained by the Darwinian theory. The objections to Darwinism are considered, the matter of the isolation of deviating individuals being considered one of the more important. The closing lectures deal with post-Darwinian theories, notably those of WALLACE, NÄGELI, and DEVRIES, while one lecture is devoted to Neolamarckism. At the close is an excellent bibliography. LOTSY's volumes form an indispensable compendium of information, not alone to students of evolution, but to all biologists, and particu-

[3] BOT. GAZETTE **42**:60–61. 1906.

[4] LOTSY, J. P., Vorlesungen über Descendenztheorien mit besonderer Berücksichtigung der botanischen Seite der Frage, gehalten an der Reichsuniversität zu Leiden. Zweiter Teil. 8vo. pp. vi+420. *pls. 13. figs. 101.* Jena: Gustav Fischer. 1908. *M* 12; geb. *M* 15.

larly to all botanists, since this work presents especially the botanical aspects of the evolutionary theories; most former works of similar character have had a strong zoological bias, which has resulted in a most imperfect consideration of botanical material.—H. C. COWLES.

MINOR NOTICES

Monograph of Oenothera.—LÉVEILLÉ[5] has issued another fascicle of his monograph of the genus Oenothera, which includes the Onagras *O. Lamarckiana*, *O. biennis*, *O. grandiflora*, and related forms. His work on most of the species has been entirely from herbarium material, with the result that the process of "lumping" rather than "splitting" the species is carried to an extreme. Numbers of species are combined into larger groups, for which new names are proposed. For example, *O. pyramidalis* Lévl. is to include *O. rhombipetala* Engelm. and Gray, *O. heterophylla* Spach, and several others. Similarly, *O. polymorpha* Lévl. includes *O. mollissima* L., *O. longiflora* Jacq., *O. nocturna* Jacq., and a number of others. The "lumping" process reaches an extreme, however, in the treatment of *O. biennis*, *O. Lamarckiana*, and related forms. All are gathered into one comprehensive "species," *O. communis* Léveillé, having three "races": *biennis* L., *Vriesiana* Lévl., and *japonica* Guffroy. The last is a Japanese form with triangular seeds. The race *Vriesiana* includes *O. Lamarckiana* and all its mutants, and one or two other species. The race *biennis* L. includes such well-recognized species as *O. muricata* L., *O. parviflora* L., *O. Hookeri* Torr. and Gr., *O. Oakesiana* Robb. and Wats., *O. strigosa* Rydb., *O. cruciata* Nutt., and a number of others. Many of these have been shown to come true in cultures, and the types are sufficiently distinct to be easily recognizable even as very young seedlings. The present *O. biennis* L., after excluding all the segregates, is probably still considered sufficiently "polymorphic" by most American botanists.

DEVRIES has emphasized the necessity of differentiating between elementary species and Linnean species, the latter only being recognized in the floras and manuals, and the elementary species being enumerated in their subordinate rank. But the indiscriminate "lumping" practiced here far surpasses the necessities of even the manuals.

Having thus combined this host of forms into one "species" (*O. communis*), LÉVEILLÉ proceeds to argue that mutation accounts for the origin of races, but that the origin of species is another problem. He states that he grew *O. Lamarckiana* and several of the mutants in his garden for five years, from seeds of DEVRIES, and reports various wonderful transformations of one form into another. DEVRIES's investigations have been verified abundantly by later careful workers, so that LÉVEILLÉ's statements need not be taken seriously, especially since he made no attempt to prevent crossing. He naïvely states that the plants were grown in two separate gardens, and that, in one case at least, the forms were sufficiently distant to prevent crossing!—R. R. GATES.

[5] LÉVEILLÉ, H., Monographie du genre Oenothera (with the collaboration of CH. GUFFROY). pp. 339–408. Le Mans. 1909.

Vegetationsbilder.[6]—The sixth and seventh parts of the seventh series of KARSTEN and SCHENCK'S well-known work reproduces twelve photographs of typical plant formations in East Bolivia, with text by TH. HERZOG. The illustrations include studies from the rain forest, the xerophytic mountain slope, the scrub, and desert societies. The series is completed in an eighth part illustrating the plant formations of Danish West Greenland in six photographs, with descriptive text by M. RIKLI. The illustrations include type areas from birch forest, heath, bog, arctic meadow, and polster societies.

The eighth series of the same work is begun with six plates from photographs by FRANZ SEINER, who also contributes the descriptive text. The plates are of the same superior quality which characterizes the entire series, and illustrates the vegetation of the dry steppes of northern and central Kalahari. Small leguminous trees form a conspicuous part of the vegetation, and among the species illustrated are *Copaifera coleosperma*, *C. mopane*, *Acacia haematoxylon*, *A. hebeclada*, *A. detinens*, and *A. horrida*, the last overgrown with *Loranthus Dregei*.—GEO. D. FULLER.

Das Pflanzenreich.[7]—Part 40 consists of a monographic presentation of the Papaveraceae by Dr. FRIEDRICH FEDDE. About 80 pages, devoted to a general account of the family, precede the taxonomy. The author divides the family into three subfamilies, namely Hypecoideae, Papaveroideae, and Fumarioideae; the first two only are elaborated in the present volume. Twenty-six genera are treated, to which are referred over 400 recognized species and numerous varieties; of these about 30 species and approximately 70 varieties are new to science. New species are described in Glaucium, Papaver, Platystemon, and Roemeria. Excellent keys precede the enumeration and description of species; bibliography and exsiccatae are very fully given; and numerous illustrations materially supplement the text. The work is an exhaustive and authoritative treatise of this interesting group of plants.—J. M. GREENMAN.

Symbolae Antillanae.[8]—The second fascicle of volume VI of this scholarly work continues the presentation of the Solanaceae by O. E. SCHULZ, in which 38 species of Solanum are recorded, 4 species and 5 varieties being new to science. The genus Cestrum is represented by 19 species. Professor URBAN contributes an article on the high mountain flora of Sto. Domingo, and A. COGNIAUX begins a detailed consideration of the Orchidaceae, treating 26 genera to which are referred 176 species; new species are described in Spiranthes, Pseudocentrum, Stelis,

[6] KARSTEN, G., AND SCHENCK, H., Vegetationsbilder. Series vii, parts 6–8. Text and *pls. 31–48;* Series viii, part 1. Text and *pls. 1–6.* 4to. Jena: Gustav Fischer. 1910. *M* 4 per part.

[7] ENGLER, A., Das Pflanzenreich. Heft 40 (IV. 104). Papaveraceae-Hypecoideae et Papaveraceae-Papaveroideae von FRIEDRICH FEDDE. pp. 430. *figs. 43* (*532*). Leipzig: Wilhelm Englemann. 1909. *M* 21.60.

[8] URBAN, I., Symbolae Antillanae seu fundamenta florae Indiae Occidentalis. Vol. VI, fasc. 2. pp. 193–432. Leipzig: Fratres Borntraeger. 1909.

Pleurothallis, Habenaria, Pogonia, and Microstylis. The descriptions of species in each genus are preceded by determinative keys.—J. M. GREENMAN.

North American flora.[9]—Volume IX, part 3, of this work consists of an elaboration of the Agaricales as follows: Boletaceae and Chantereleae by W. A. MURRILL, and Lactarieae (pars) by G. S. BURLINGHAM. New species are described in Gyroporus (1), Ceriomyces (5), and Suillellus (1). Three new monotypic genera of the Agaricaceae are proposed, namely, *Polyozellus*, *Plicaturella*, and *Chlorophyllum*.—J. M. GREENMAN.

NOTES FOR STUDENTS

Graft hybrids.—The question of graft hybrids continues to grow in interest. WINKLER's previous papers on this subject have already been reviewed in this journal.[10] His latest contribution[11] deals with the second and later generations of the graft hybrids between *Solanum nigrum* and *S. lycopersicum*, and with the chromosome numbers in these forms. It is found that the different graft hybrids fall into a series resembling most one or the other parent, in the following order: *S. nigrum*, *S. Gaertnerianum*, *S. Darwinianum*, *S. tubingense*, *S. proteus*, *S Koelreuterianum*, *S. lycopersicum*, the first three graft hybrids being most like *S. nigrum* and the last two nearest *S. lycopersicum*. In the F_2 the first three revert to *S. nigrum*, while the other two produce the tomato. Large numbers of offspring were grown in some cases, e.g., 1200 individuals in the F_2 of *S. tubingense*, all of which without exception were pure *S. nigrum*. The F_3 and F_4 and later generations continued true *S. nigrum*. Regenerated adventive shoots also reverted only to the nightshade, but in the case of *S. proteus* reversions to both parents occurred, and in this and other cases various chimeras appeared from adventive shoots. *S. Gaertnerianum* is almost sterile and shows little tendency even to parthenocarpy, which is more frequent in some of the others.

Crosses of the graft hybrids with the parent they most resembled gave similar results, e.g., *S. tubingense*×*S. nigrum* gave only *S. nigrum*. Crosses with the more distant parent, as *S. tubingense*×*S. lycopersicum*, failed to produce seeds, although parthenocarpy frequently occurred.

Regarding the chromosomes, in *S. lycopersicum* the numbers were 12 (haploid) and 24 (diploid), and in *S. nigrum* 36 and approximately 72, therefore three times as many. If a fusion of nuclei took place in the production of the graft hybrid, one might expect the latter to have 72+24 (96) chromosomes unless a regulation in the number afterward occurred; but this number is not found. Instead, *S. tubingense*, *S. Darwinianum*, and *S. Gaertnerianum* were found to have 36 chromosomes in their pollen mother cells (after reduction), while the other two

[9] North American flora. Vol. IX, part 3. pp. 133–200. New York Botanical Garden. 1910.

[10] BOT. GAZETTE **47**:84, 250; **48**:478. 1909.

[11] WINKLER, HANS, Ueber die Nachkommenschaft der Solanum-Pfropfbastarde und die Chromozomenzahlen ihrer Keimzellen. Zeit. Bot. **2**:1–38. 1909.

possessed 12 chromosomes. Therefore in both cases the numbers are the same as in the parents to which they revert in the F_2. This of course does not eliminate the possibility that a fusion of nuclei had occurred, and that afterward a regulation in the number took place, perhaps just previous to reduction or at the time of reduction.

STRASBURGER[12] has made a number of Solanum grafts according to WINKLER'S method, and examined the growing tissues along their line of union, to determine whether cell fusions take place. He found no indication that such is the case, but of course negative evidence in such a matter is inconclusive, for the graft hybrids are rare at best. STRASBURGER concludes that WINKLER'S cases are really complicated chimeras, in which the tissues of the two parents are intimately blended in the growing points. He proposes to call them "hyperchimeras," and cites various analogous cases of very intimate relationship, such as between parasite and host.

WINKLER, however, is still convinced that these cases are true graft hybrids, although concurring in STRASBURGER'S opinion that *Cytisus Adami*, which STRASBURGER[13] showed to have the same number of chromosomes as its parents, is a chimera. He proposes to determine the chromosome number in the somatic cells of the Solanum hybrids to see whether it is higher than in the germ cells, and contends that, even though no fusion of nuclei occurs, in its absence the effect of the cytoplasm of one type of cell upon the other will be necessary to explain the production of the characters of the graft hybrids. This view scarcely seems necessary to explain the present facts, but WINKLER'S further cytological papers to determine what actually occurs will be awaited with interest. It is hoped that full accounts, with figures, will be forthcoming.

BAUR[14] has reiterated recently his belief that these forms are explainable as periclinal chimeras, varying in the arrangement of the layers in the growing point, and thinks that the case of Crataegomespilus can be explained in the same way.

The fact that in these Solanums the number of chromosomes is so unlike adds much to the interest of the situation.—R. R. GATES.

Sexuality of the rusts.—The differences of nuclear behavior in the rusts as described by BLACKMAN and CHRISTMAN have led KURSSANOW[15] to investigate a similar form. According to BLACKMAN,[16] in the caeoma of *Phragmidium violaceum* the nucleus of a vegetative cell passes into that of a specially differentiated female

[12] STRASBURGER, E., Meine Stellungnahme zur Frage der Pfropfbastarde. Ber. Deutsch. Bot. Gesell. **27**:511–528. 1909.

[13] ———, Ueber die Individualität der Chromosomen und die Pfropfhybriden-Frage. Jahrb. Wiss. Bot. **44**:482–555. *pls. 5–7. fig. 1.* 1907.

[14] BAUR, E., Pfropfbastarde, Periklinalchimären, und Hyperchimären. Ber. Deutsch. Bot. Gesell. **27**:603–605. 1910.

[15] KURSSANOW, L., Zur Sexualität der Rostpilze. Zeit. Bot. **2**:81–93. *pl. 1.* 1910.

[16] BLACKMAN, V. H., On the fertilization, alternation of generations, and the general cytology of the Uredineae. Annals of Botany **18**:323–373. 1904.

cell, which bears an abortive trichogyne-like organ. According to CHRISTMAN,[17] on the other hand, in the caeoma of *Phragmidium speciosum* we have the conjugation of two equally differentiated cells. BLACKMAN, therefore, believes that a heterogamous condition obtains, while CHRISTMAN describes an isogamous one. In the subsequent papers of these two authors this essential difference is still manifest (see BLACKMAN and FRASER,[18] CHRISTMAN[19]). In a more recent publication, OLIVE[20] compares these differences and takes a middle ground, believing that both BLACKMAN and CHRISTMAN are in particular cases right. In four forms of the same caeoma type as Phragmidium, OLIVE finds that the cells conjugate as described by CHRISTMAN, but that one is larger than the other, that is, a male and a female cell conjugate, the male cell lying a little below the female, which is always surmounted by a trichogyne-like abortive cell, while the male cell has no such abortive portion. During conjugation a broad opening may be present between the two cells, allowing the protoplasmic contents to mix, as described by CHRISTMAN, or only a narrow opening between the two cells may be present, so that only the nuclei of the male cells pass into the female cells, a condition which OLIVE believes to agree with BLACKMAN'S description. The two phenomena may be observed in the same plant. OLIVE thus seeks to harmonize the views of BLACKMAN and CHRISTMAN.

KURSSANOW has investigated a caeoma, which he found growing on *Rubus saxatilis* near Moscow, and which he regards as the same or a very similar form to the caeoma studied by CHRISTMAN. He regards CHRISTMAN'S studies as too brief to be conclusive, and endeavors to extend the observations. The cells of the vegetative hyphae and haustoria are uninucleate, the nuclei being comparatively large and distinct. Numerous spermatogonia are present on both surfaces of the leaves, whose origin and development are described, with the result that the cells are always uninucleate. The caeomas appear only on the under sides of the leaves. The formation of a sort of palisade arrangement of uninucleate large terminal cells as described by CHRISTMAN is also described by KURSSANOW. There is a conjugation in pairs of these cells, which he regards as equal in size. There is no condition found which could be made to agree with OLIVE'S description, but his account is in complete harmony with CHRISTMAN'S results. The differences in the methods of conjugation as described by BLACKMAN, CHRISTMAN, and OLIVE, this author believes may be due to differences in the forms investigated

---

[17] CHRISTMAN, A. H., Sexual reproduction in the rusts. BOT. GAZETTE **39**:267–274. 1905.

[18] BLACKMAN, V. H., AND FRAZER, MISS H. C. I., Further studies on the sexuality of the Uredineae. Annals of Botany **20**:35–48. 1906.

[19] CHRISTMAN, A. H., The nature and development of the primary uredospore. Trans. Wis. Acad. Sci. **25**:517–526. 1907; Alternation of generations and the morphology of the spore forms in the rusts. BOT. GAZETTE **44**:81–101. 1907.

[20] OLIVE, E. W., Sexual cell fusions and vegetative nuclear divisions in the rusts. Annals of Botany **22**:331–360. 1908.

or to a pathological nuclear migration, as suggested by CHRISTMAN, which KURSSANOW has often observed accompanying the normal sexual process. He believes the sterile cells, which are equally well formed on all of the fertile ones, are not to be regarded as trichogynes. They appear to act only as buffers between the epidermis and the fertile cells. He strongly doubts any connection or direct relationship of the rusts to the red algae.—J. B. OVERTON.

Animal ecology.—It is pleasant to note greatly increased activity along ecological lines on the part of zoologists. Contributions that should have been noticed here long since are found in papers by SHELFORD, RUTHVEN, and HANKINSON. SHELFORD[21] has considered the relation of tiger beetles to plant succession, and has found that there is quite as definite a succession of forms, when the physiography changes, as has been recorded for plants. For example, certain species of Cicindela characterize the beach of the dune region near Chicago, while others take their place as the dunes develop, and still others as the dunes become established. Similar succession stages are given for depressions. No species seems to be fitted for life in the climax mesophytic forest of the region.

RUTHVEN,[22] after presenting the environmental features of certain regions of New Mexico and Arizona together with full descriptions of the animals collected, gives an exceedingly interesting summary. It is concluded "that each set of environmental conditions which is marked out by a distinct plant association has a definite reptile fauna," and that the sharpest line between the animal associations is that corresponding to the line between the pinyon-cedar association and the more arid treeless associations. It is also noted that continued denudation may be expected to result in the increased development of the arid associations. Plant ecologists, who as a class have long recognized the fundamental errors in MERRIAM'S zonal classification as applied to plants, will be interested in observing RUTHVEN'S conclusion, based on reptiles and amphibians, that "it seems advisable not to stretch any one zone over the entire continent." The paper is illustrated by a number of halftones showing characteristic animals and habitats.

HANKINSON[23] has given a detailed account of a biological survey of a small Michigan lake. Following an account by DAVIS on the physiography and geology of the region, HANKINSON gives a detailed statement concerning the various field stations and their characteristics, nine kinds of habitats being recognized between the shore and deep water. After an ecological account of the fish fauna by HANKINSON, DAVIS treats of the flora and its ecological features, giving a list of the

[21] SHELFORD, V. E., Preliminary note on the distribution of the tiger beetles (Cicindela) and its relation to plant succession. Biol. Bull. **14**:9–14. 1907.

[22] RUTHVEN, A. G., A collection of reptiles and amphibians from southern New Mexico and Arizona. Bull. Amer. Mus. Nat. Hist. **23**:483–604. 1907.

[23] HANKINSON, T. L., A biological survey of Walnut Lake, Michigan, with chapters on the physiography, geology, and flora of the region by CHARLES A. DAVIS, and a paper on the aquatic insects of the lake by JAMES G. NEEDHAM. Rep. Mich. Geol. Surv. **1907**:157–288. 1908.

aquatic plants collected. HANKINSON then considers the lake fauna in general, concluding with a summary of the life and the life conditions of the littoral and abyssal parts of the lake. Following an appendix with diversified matter are a large number of excellent plates, most of which illustrate the characteristic lake habitats.—HENRY C. COWLES.

Various colored lights and photosynthesis.—It is fast becoming evident that the telling work in the advance of plant physiology is to be done by men who are prepared to work by the exact methods of the physicist and chemist. One feels that KNIEP and MINDER[24] have indicated the way for the final settlement of the perplexing question of the part played in photosynthesis by the various portions of the spectrum. They have given great attention to developing suitable screens and to accurate methods of determining the energy values of the light thus obtained. This is apparently done with exact knowledge and application of the physics involved. Although it is unfortunate that the rate of synthesis must be tested by the bubble method, this seems to be the least objectionable method available. In the light used all heat, as well as infra-red and ultra-violet rays, was cut out by distilled water containing traces of potassium bichromate and ammoniacal copper sulfate. The screens were specially made colored glass, the spectra of which were fully studied. The measurements were made between 11 A.M. and 12:30 P.M. by means of reflected sunlight. The present paper reports the results of measurements of the synthetic value of equal energy intensities (as shown by the blackened thermopyle) of red light (620 $\mu\mu$ toward the infra-red), blue light (523.8 $\mu\mu$ toward the ultra-violet—little transmission to the left of 509 $\mu\mu$), and green light (512–524 $\mu\mu$). Green gave no photosynthesis. Red and blue of equal energy value gave equal photosynthesis. This finding is quite in contrast with the current conception of plant physiologists who hold that the blue end of the spectrum plays little part in the process. This view is due to the fact that the blue screens commonly used reduce to a much greater degree the energy intensity than do the red. It is evident that the relative value of the two portions of the solar spectrum is approximately proportional to the relative energy amounts of the two portions. These relative amounts vary with the time of day, cloudiness, humidity, altitude, etc., the red, of course, being in general greater, but the blue far from negligible. The writers mention that this is only the first step in this important work. They hope by the use of suitable screens, or prism-resolving methods, to study the photosynthetic value of each portion of the spectrum on the basis of its energy value, and to construct a complete curve of this value.—WM. CROCKER.

Germination of the seeds of certain parasites.—The germination of the seeds of various parasites belonging to the Rhinanthaceae has been rather extensively investigated. Two of the more recent studies illustrate two of the more prominent

[24] KNIEP, H., UND MINDER, F., Ueber den Einfluss verschiedenfarbigen Lichtes auf die Kohlensäureassimilation. Zeit. Bot. 1:619–650. 1909.

types of development. In the hemiparasite *Melampyrum pratense*, GAUTHIER[25] found that the seeds germinated without any stimulus from other organisms, and that the seedlings had a short independent existence, during which the roots branched freely. Root hairs developed in abundance, many of them consisting of two or three cells, but before the food stored in the seed became exhausted, the root hairs began to be replaced by haustoria which penetrated the roots of the host. Trees with mycorhiza upon their roots were the hosts, the beech much more frequently than other species. Failure to effect a contact with a suitable host resulted in the early death of the seedlings. The investigator stated that the seeds, in common with those of Rhinanthus, Euphrasia, and Pedicularis, would not germinate if allowed to become dry.

HEINRICHER studied the same species,[26] obtaining results essentially similar to the above, but he claims[27] that in general the seeds of Rhinanthus, Euphrasia, and Pedicularis are not so sensitive to desiccation, but retain their vitality for several months, although some of them germinate only during the spring. He has studied the group extensively, and in the recent article summarizes the life history of a highly specialized type discussed in a former paper.[28] The seeds of Tozzia germinate only with the chemical stimulus afforded by the roots of the host, its cotyledons never appear above the soil, and for two or three years it is a subterranean holoparasite. Finally it sends up an aerial shoot which becomes green, flowers, and produces seeds. As the seeds mature the plant dies, each individual flowering once only.—GEO. D. FULLER.

Vegetation of the Danish West Indies.—As a result of further study of the halophytic vegetation of the Danish West Indian Islands, BØRGESEN[29] has modified somewhat his former classification of the plant societies involved, and now distinguishes a hydrophytic vegetation composed of sea grass and algae, and a halophytic vegetation embracing the muddy soil vegetation, the sand strand vegetation, and the rocky coast vegetation. The muddy soil vegetation is made to include the mangrove, Salicornia, and Conocarpus formations. The author places emphasis upon the influence of exposure and soil consistency as the deciding factors in limiting the mangrove formation, which he defines as a formation of treelike evergreen plants growing on the sheltered shores, partly in shallow salt or

---

[25] GAUTHIER, L., Sur le parasitisme du *Melampyrum pratense*. Rev. Gén. Bot. **20**:67–84. 1908.

[26] HEINRICHER, E., Die grünen Halbschmarotzer, V. Melampyrum. Jahrb. Wiss. Bot. **46**:273–376. 1909.

[27] ———, Germination des graines des plantes parasites. Rev. Gén. Bot. **21**:329–337. 1909.

[28] ———, Die grünen Halbschmarotzer. III. Bartschia und Tozzia. Jahrb. Wiss. Bot. **36**:665–752. 1901.

[29] BØRGESEN, F., Notes on the shore vegetation of the Danish West Indian Islands. Bot. Tidssk. **29**:201–259. *pls. 3–6*. 1909.

brackish water, partly on low-lying soil which is rarely, sometimes never, covered by salt or brackish water.

While visiting the islands in 1906, he noted that the extensive mangrove forest occupying one of the larger lagoons had perished. The explanation seems to be that in 1899 the island was ravaged by a serious hurricane, which so violently agitated the sea that large quantities of clay and gravel were washed into the lagoon and deposited as a layer several inches thick. This caused so sudden a change in the soil constitution as to destroy the forest, although there were clear indications that the same formation would eventually reclaim much of the area formerly occupied.

The small trees growing in drier situations seem sufficiently distinct from the mangroves to be regarded as a distinct society, which is named the "Conocarpus formation," after the most conspicuous member. This formation is regarded as very nearly the ecological equivalent of the Nipa formation of SCHIMPER in the Indo-Malayan strand flora. The vegetation of the sand strand exposed to the wind includes the well-known Pes-caprae formation, an aggregation of shrubs designated the Tournefortia formation, and the forest Coccoloba-Manchineel formation. The anatomy of several of the strand plants has been investigated, and some further studies made of the more xerophytic vegetation of the rocky coasts.—GEO. D. FULLER.

Loess and plant societies.—The intimate relation between physiographic plant ecology and geology receives emphasis in the evidence deduced by SHIMEK[30] to prove that the irregular distribution of loess deposits has been effected by the character of formerly existing plant societies. Vegetation is known to play an important part in pulverizing the soil, affording anchorage for fine wind-blown particles, and finally in returning its own substance to the soil in a finely subdivided condition. Thus well-drained areas supporting heavy mesophytic vegetation accumulated fine material now appearing as moderate deposits of loess of uniform quality. Xerophytic societies of unequal density resulted in inequality and irregularity of the deposits, while hydrophytic areas, as well as those of such elevation or aridity as to be without vegetation, collected no loess. The very diverse aspect of the plant societies on the east and west sides of large streams like the Mississippi is found to correspond exactly with the difference in quality and distribution of their loess deposits. The interstratification of sand the author believes to have resulted from periods of very scanty vegetation caused by climatic changes.

While the thesis is essentially an argument in support of the eolian hypothesis for the formation of loess, it has many points of interest for the ecologist; for should further evidence prove the soundness of the author's views, it would afford a useful key to the distribution of plant societies during recent geologic times. The author finds the present edaphic influence of loess evident in the vegetation, even

[30] SHIMEK, B., The genesis of loess a problem in plant ecology. Proc. Iowa Acad. Sci. **15**:57–74. *pls.* 3–7. 1909.

where the surface overwash has hidden the character of the substratum. The forest on the loess is dominated by *Quercus alba* and *Q. ruba*, while the adjacent sandy areas are covered by the more xerophytic *Q. velutina* and *Q. macrocarpa*.—GEO. D. FULLER.

Air chambers of Ricciaceae.—Miss HIRSH,[31] under the direction of Dr. E. J. DURAND, has examined a number of species of Riccia to determine the correctness of the statement of BARNES and LAND that the air chambers of Marchantiales arise invariably by the splitting of internal walls. She finds that in the Ricciaceae the statement is true only for *Riccia natans* and *Riccia fluitans*, and that in all other species which she examined the air chambers arise according to the method described by LEITGEB, and cites as proof three figures of *Riccia Frostii*. To critical students of the group these figures do not furnish conclusive evidence one way or the other, for they are made in such a manner that the relation to each other of the rows of the cells back of the growing point cannot be made out with any certainty. In *fig. 4* of *Riccia Frostii* the first air chambers can as easily be interpreted as having split from within the thallus and having just reached the surface, as that the cells have become papillate. In fact, the contour of the section drawn seems to show that all the filaments actually originated by splitting and intercalary growth. The same is true in a more marked degree of *fig. 5*, and less so of *fig. 6*. In the latter figure the arching of the superficial cells due to turgor is interpreted by Miss HIRSH as the beginning of the papillate outgrowths of LEITGEB. Such investigations should be preceded by a careful study of the development of the thallus from the growing point, and there should be a clear conception of the arrangement of the cells which result from this growing point. While the style of the drawings is admirable, the position of cells and cell walls shows that such study must have been neglected in this case.—W. J. G. LAND.

Light and germination.—KINZEL[32] has devised apparatus that answers all objections to his former methods, which indicated that light favored or was even necessary for the germination of various seeds. Both illuminated and darkened after-ripened seeds of *Veronica Anagallis* were kept in germinators at a constant temperature of 16°7 C. Within a week 100 per cent of the illuminated cultures had germinated, while none of the darkened ones grew even after three months. He lists 63 species that germinate only in light, of which the following are examples: *Scheuchzeria palustris*, *Luzula albida*, *Thalictrum angustifolium*, *T. aquilegiifolium*, *Drosera rotundifolia*, *D. anglica*, *D. intermedia*, *Primula pubescens*, *P. spectabilis*, *Verbascum Thapsus*, *V. nigrum*, *Mimulus luteus*, *Veronica Anagallis*, and *Campanula rotundifolia*. He does not state whether high temperatures will dispose of the necessity of light, as is the case with various fern spores. It also

[31] HIRSH, PAULINE E., The development of air chambers in the Ricciaceae. Bull. Torr. Bot. Club **37**:73–77. *figs. 6.* 1910.

[32] KINZEL, W., Lichtkeimung: Erläuterungen und Ergänzungen. Ber. Deutsch. Bot. Gesell. **27**:536–545. *pl. 19.* 1909.

appears that he used the very low temperature of 16°7 C. He finds that some seeds germinate much better in darkness than in light, for example, *Leucojum vernum*, Asphodelus, *Anthericus ramosus*, *Paris Smilacina*, Polygonatum, and Veratrum. Phacelia and *Nemophila insignis* germinate very slowly and sparingly in white light, more rapidly and abundantly in darkness, and very quickly and completely in blue light. KINZEL finds the favorable action of the light is not due to the actinic rays, for in many cases the rays exerting least actinic power are most effective in bringing about germination.—WILLIAM CROCKER.

Heterotypic mitosis in Lilium.—In a paper on the prophase of the heterotypic mitosis in the embryo sac mother cell, MOTTIER[33] describes the effect of fixing fluids and traces the early development of the heterotypic spirem in the megaspore mother cell of *Lilium Martagon* and *L. candidum*. The following conclusions are reached: (1) Previous to synapsis a single nuclear thread is developed, which in many cases can be demonstrated clearly as a definite spirem with somewhat regular and uniform chromatin granules. (2) There is no union side by side of two distinct chromatin spirems before or during synapsis, which is regarded as a normal process, but the greater compactness of the balled-up mass may be due partly to the reagent. (3) The hollow spirem following synapsis is double, due to the longitudinal fission, which as a rule becomes completely obscured before the transverse segmentation. (4) The first mitosis, therefore, separates transversely the two members of the bivalent chromosome. (5) The heterotypic mitosis is thus a reduction division, and if one chromosome differs from another potentially or otherwise, it is also qualitative. (6) In the presynaptic phase, that the chromatin may appear as large clumps instead of smaller and uniform granules has been suggested as being due in part to the fixing fluids, the finer and more uniform granules being nearer the normal. The wide divergence of the halves of the chromatin thread appearing occasionally in the stage of the hollow spirem may also be due in part to the reagent.—SHIGÉO YAMANOUCHI.

Germination of zygotes of Spirogyra.—The germination of zygotes of *Spirogyra jugalis* Ktzg. has been studied by KARSTEN.[34] The germinating stage of the zygotes was obtained during November and December, and some of the cytological results are as follows: Two nuclei in the zygote are in close contact, the membranes between them staining faintly and evidently becoming dissolved. In the fusion nucleus two nucleoli derived from two gamete nuclei are seen for a time, but they finally fuse. The interior of the fusion nucleolus is vacuolized, but the outer portion remains and stains deeply. At the first division of the nucleus the synaptic stage is represented by a peculiar and irregular massing of the nucleolar membrane and a peculiar elongated condition of the nucleolus, which is now

[33] MOTTIER, DAVID M., On the prophases of the heterotypic mitosis in the embryo sac mother cell of Lilium. Annals of Botany **23**:343–353. *pl. 23*. 1909.

[34] KARSTEN, G., Die Entwicklung der Zygoten von *Spirogyra jugalis* Ktzg. Flora **99**:1–11. *pl. 1*. 1908.

directly surrounded by the cytoplasm. The nucleolus then gradually assumes a rounded form, in which are seen 14 deeply stained tetrads that differentiate later into 28 chromosomes. The chromosomes become united in pairs and 14 bivalent chromosomes are established. The spindle is at first multipolar, but gradually becomes bipolar. The daughter chromosomes grouped at the poles, after the first division, again take the form of nucleoli in the daughter nuclei. In the second division 14 chromosomes are clearly distinguishable in the polar view of the equatorial plate. Consequently in *Spirogyra jugalis* 14 is the haploid number of chromosomes and 28 the diploid number.—SHIGÉO YAMANOUCHI.

Oxidizing enzymes.—EULER and BOLIN[35] have published a third paper on oxidizing enzymes. They have developed a method for the quantitative determination of peroxidase. They have also devised methods for the purification of the peroxidase of the horseradish, which gives a far superior product to that obtained by BACH and CHODAT. The most highly purified product showed 10.4 per cent of nitrogen and 2.5 per cent of ash. Contrary to the belief of many, the action of this substance cannot be attributed to the trivalent iron and chinons it contains, but it is a true enzyme. They believe the peroxidases of various plants are not identical.

The oxidase of *Rhus vernicifera* was purified and compared with the oxidase of Medicago, which they have already studied in great detail.[36] The latter they have shown to be a mixture of calcium salts of various oxy-acids and it is not destroyed by boiling in water. The former contains nitrogen and is very sensitive to heat. Contrary to the contention of many, they find that the action of Rhus oxydase cannot be attributed to the joint action of contained manganese and hydroxyl, for it is equally effective in slightly acid, neutral, and slightly basic solutions.

These investigators, through their excellent chemical training, are making valuable contributions in this important field of plant physiology.—WM. CROCKER.

The giant form of Primula sinensis.—GREGORY[37] has examined the giant form of *Primula sinensis*, thinking it might have the tetraploid number of chromosomes, as Miss LUTZ and the reviewer found to be the case in *Oenothera gigas*. A doubling, however, does not occur, the numbers being 12 and 24, as in the ordinary form of *Primula sinensis*. It should be said that this giant Primula differs from the ordinary form in practically no respect except the larger size of its organs, while in *O. gigas* many of the characters have been sharply modified from those of *O. Lamarckiana*. While GREGORY finds the number of the chromosomes unchanged, he believes that the chromosomes are larger in the giant form,

---

[35] EULER, H., UND BOLIN, IVAN, Zur Kenntnis biologisch wichtiger Oxydationen. III. Zeit. Physiol. Chemie **61**:72–92. 1909.

[36] *Ibid.* **61**:1–15. 1909.

[37] GREGORY, R. P., Note on the histology of the giant and ordinary forms of *Primula sinensis*. Proc. Cambridge Phil. Soc. **15**[3]:239–246. *pl. 10.* 1909.

although this is difficult to prove. The cells and nuclei are found to be distinctly larger, as shown by measurements of nuclei of the pollen mother cells and of various other tissues. Drawings from transverse sections of the stems show clearly the larger size of the cells in the giant form. He concludes that "the character of giantness manifests itself in the cells themselves and not merely in the plant as a whole."

The reviewer has shown[38] that in *Oenothera gigas* the cells and nuclei are constantly larger than in *O. Lamarckiana*, but that the ratio of increase varies in different cases, the sizes being 1.5:1, 2:1, 3:1, or even more, though apparently constant for each tissue examined.—R. R. GATES.

Secondary growth in monocotyledons.—Evidence against the significance of histogenic layers in the stem apex continues to accumulate. SCHOUTE[39] dealt a severe blow to HANSTEIN'S theory when he showed that in Hippuris the plerome gives rise not only to the central cylinder but also to part of the cortex. As to vascular cryptogams, CAMPBELL[40] found that the vascular bundles of Equisetum originate from the cortical region. A monocotyledon has recently been studied by CARANO,[41] who finds that in the young stem and leaf of *Yucca aloifolia* it is impossible to distinguish between plerome and periblem. The author concludes that in this stem the existence of two distinct regions, central cylinder and primary cortex, is absolutely unfounded. It may be objected, however, that in this case there is merely negative evidence, which will not settle the question for the monocotyledons. In the stem of this plant there is nothing corresponding to the pericycle of the dicotyledons, and the meristematic zone which gives rise to the secondary tissues is continuous with the apical meristem. Hence this zone is considered to be primary at the outset, though the cambial activity later spreads outward to mature cells, when the meristem and its products of course become secondary. The permanently active layer so established produces vascular bundles and parenchyma internally, and parenchyma externally.—M. A. CHRYSLER.

Affinities of an alpine flora.—Following the glacial relic theory postulated by GRAY and elaborated by HOOKER and others, HARVEY[42] has studied the vascular flora of Mt. Ktaadn, Maine. Four distinct elements are distinguished: (1) the arctic-relic, (2) the pre-glacial alpine, (3) the endemic, and (4) the subalpine-lowland. Of these the last is regarded as not truly alpine, while the endemic flora consists of the single initial endemic *Carex Grahamii* and the relic endemic

[38] GATES, R. R., The stature and chromosomes of *Oenothera gigas* DeVries. Arch. f. Zellforsch. **3**:525–552. 1909.

[39] SCHOUTE, J. C., Die stelär Theorie. Jena. 1903.

[40] CAMPBELL, D. H., Affinities of the genus Equisetum. Amer. Nat. **39**:273–285. 1905.

[41] CARANO, E., Su le formazioni secondarie nel caule delle Monocotiledoni. Annali di Botanica **8**:1–42. *pls. 1–4.* 1910.

[42] HARVEY, LEROY H., The floristic composition of the vascular flora of Mount Ktaadn, Maine. Mich. Acad. Sci. Report **11**:37–47. 1909.

Euphrasia Oakesii. The remaining arctic-alpine flora consists of 118 species, all but four of which are also found upon Mt. Washington. In the various tabular presentations of the affinities of this alpine flora, attention is directed to the ecological as well as the floristic similarity of mountains, coast, bog, and arctic habitats, and various arctic and alpine floristic areas are compared with Mt. Ktaadn. From the large number of common species (56 per cent), arctic Europe is considered to have been the center of distribution of the Ktaadn alpine flora, while its glacial migration seems to have been by the Greenland-Laborador route. Over 75 per cent of this flora is of arctic affinity.—GEO. D. FULLER.

Diffusion stream in plant organs.—RYWOSCH[43] continues his work on the movement of food materials in plant organs. The movements of course obey the laws of diffusion, a given substance moving in the direction of its lowest concentration. The thing of interest in his work is the discovery of various means by which the gradient is maintained. Two illustrations will show the nature of the results. If the cuticle is removed from any side of a starch-free pine needle and the needle placed in a 9 per cent sugar solution, the first deposit of starch is not on the side of the removed cuticle, where of course the sugar is most concentrated, but in the cells on the distal side. The entrance on one side through irritability leads to the more complete condensation of the sugar on the opposite side, thus maintaining a falling gradient and a continual diffusion stream of the sugar toward the distal side. In the cotyledons of the pea, also, he finds that the sugar is more completely condensed as the vascular bundle is approached, thus maintaining a falling gradient and continuous sugar diffusion toward the bundle.—WM. CROCKER.

Chromatin bodies.—Miss DIGBY describes[44] a peculiar phenomenon of a constant extension of chromatin bodies during the presynaptic and synaptic stage of the first nuclear division of the pollen mother cells of *Galtonia candicans*. According to her observations, the chromatin bodies may originate as portions of the nuclear network, or of the synaptic knots, or as nucleolar buds. They are composed of linin in which chromatin is imbedded, or of nucleolar material. Those bodies that come from the chromatic portion of the nucleus retain their connection with the nucleus, by means of a fine thread, until their disintegration. The bodies that pass from the nucleolus into the surrounding cytoplasm penetrate the cell wall and enter the neighboring cell. The chromatic bodies which originate as buds from the nucleolus at first take an acid stain, but as they pass into the cytoplasm they become increasingly chromatic. It is not known whether the bodies as formed become secondarily attached to the nucleus. Regarding the significance of this phenomenon, no interpretation is given by the author.—SHIGÉO YAMANOUCHI.

[43] RYWOSCH, S., Ueber Stoffwanderung und Diffusionsströme in Pflanzenorganen. Zeit. Bot. **1**:571–591. *figs. 4.* 1909.

[44] DIGBY, L., Observations on chromatin bodies and their relation to the nucleolus in *Galtonia candicans* Decsne. Annals of Botany **23**:491–502. *pls. 33, 34.* 1909.

Forests and floods.—According to MOORE,[45] forests exert no controlling influence upon rainfall, and very little upon the flow of the water after it reaches the earth's surface. While regretting the paucity of the data, he decides that the run-off of our rivers is not materially affected by any other factor than precipitation, that high waters are not higher and low waters are not lower than formerly, nor do floods occur more frequently and continue longer than formerly. In support of his contention that forestation exerts little or no effect upon precipitation, he shows that the presence or absence of forest covering affects only a thin stratum of air over the surface of the areas in question, whereas the conditions that control precipitation are confined to a much greater altitude and one not affected by the local irregularities occurring in the lower stratum. Statistics given for the Ohio River basin, where deforestation has been great, show that the flow of water in that river has exhibited no material change for the thirty-seven years for which measurements are available.—GEO. D. FULLER.

Evaporation and plant societies.—Evaporation is regarded as a very mportant factor in determining the character of plant societies, and several ecologists, measuring it with the most accurate instruments now available, are collecting interesting data from various localities. SHAW[46] determined the amount of evaporation at various stations in the Selkirk Mountains, ranging from 800 to 2900^{m} in altitude, extending his observations over a period of 12 weeks. The maximum evaporation was at 1100^{m}, while above this altitude there was a gradual but somewhat irregular diminution.

DICKEY,[47] in a bog habitat, found evaporation in the open central area occupied by Sphagnum, Oxycoccus, and Eriophorum much greater than in the surrounding maple-alder zone, and that rainfall had a more marked effect upon the rate of evaporation in the latter situation. In the maple-alder zone the appearance of foliage in the spring and the fall of leaves in the autumn affected the rate materially.—GEO. D. FULLER.

Stem gall on Commelina.—The LEEUWEN-REIJNVAANS[48] give a discussion of the gross and anatomical character of a stem gall on *Commelina communis* L. It is caused by the larva of a lepidopterous insect, *Aegeria unijormis* Snellen. The larval chamber originates in the central axis of the stem, and the enlargement of the stem at this point is on one side, instead of radial as is the case with most galls which originate in the central axis of the stem. The enlargement is due mainly to the increase in the amount of the parenchyma tissue. The collenchyma is

[45] MOORE, WILLIS L., Influence of forests on climate and on floods. Report to Committee on Agriculture, U.S. House of Representatives. pp. 38. 1910.

[46] SHAW, C. H., Present problems in plant ecology. III. Vegetation and altitude. Amer. Nat. **43**:420–431. 1909.

[47] DICKEY, MALCOLM G., Evaporation in a bog habitat. Ohio Nat. **10**:17–23. 1909.

[48] LEEUWEN-REIJNVAAN, J. UND W., DRS. VON. Kleinere cecidologische Mitteilungen. Ber. Deutsch. Bot. Gesell. **27**:572–581. *figs. 6.* 1910.

broken on the side of the greatest enlargement, so that in cross-section it appears as a crescent partly inclosing the gall. The vascular bundles are pushed apart on the side where the swelling is greatest, and in cross-section form a crescent pattern instead of a ring. Small vascular bundles penetrate the tissues of the gall. The nutritive tissue is abundant and the sclerenchyma tissue is more abundant than in most lepidopterous galls.—MEL. T. COOK.

Light and germination.—Where light was supposed to be necessary for any considerable germination, LEHMAN finds[49] that in *Ranunculus sceleratus* other stimuli can be substituted for light. One per cent KNOP's solution gives a greater percentage of germination in darkness than distilled water in light. In contrast to wet filter paper generally used as a substratum in such experiments, earth was found to stimulate germination greatly, water extracts of earth made by boiling were less effective, and extracts made with cold water showed no stimulation. If these seeds are exposed to germinative conditions in darkness for twenty or more days, light is then not effective. The author calls such seeds "dunkelharten" or "dunkelstarren," in contrast to the "lichtharte" seeds of Nigella.[50] *Stellaria media*, indifferent to light in its germination, was greatly stimulated by such substrata as KNOP's solution and earth.—WILLIAM CROCKER.

Selaginella preissiana.—The extremely small xerophytic *Selaginella preissiana*, found in West Australia, Victoria, and Tasmania, is described by BRUCHMANN.[51] The cotyledons are slightly larger than the foliage leaves. The first dichotomy gives an erect branch about 3^{cm} high; the other branch becomes the creeping rhizome, which gives off erect shoots right and left. Growth is by means of a single polyhedral apical cell. The stem is protostelic, the hypocotyl having a single exarch protoxylem point.

In the root there are three groups of initials: periblem and plerome having a common group, and dermatogen and calyptrogen each having a group. Root hairs are wanting, and epidermis and cortex are infested with an endophytic fungus. The hyphae were observed penetrating the epidermis from the soil.—W. J. G. LAND.

Cellulose-forming enzyme.—In continuing his capillary analysis of enzymes, GRÜSS[52] claims to have found, as his most important result, a cellulose-forming enzyme which he terms cytocoagulase. The material condensed was dissolved

---

[49] LEHMAN, ERNST, Zur Keimungsphysiologie und Biologie von *Ranunculus sceleratus* L. und einiger anderen Samen. Ber. Deutsch. Bot. Gesell. **27**:476–494. 1909.

[50] KINZEL, WILHELM, Ueber den Einfluss des Lichtes auf die Keimung. "Lichtharte" Samen. Ber. Deutsch. Bot. Gesell. **25**:269–276. 1907.

[51] BRUCHMANN, H., Ueber *Selaginella preissiana* Spring. Flora **100**:288–295. *figs. 8*. 1910.

[52] GRÜSS, J., Kapillaranalyse einiger Enzyme. II. Ber. Deutsch. Bot. Gesell. **27**:313–319. 1909.

from cherry gum, and the crystalline precipitate stained red with phloxin. It is not difficult to see that his evidence is hardly conclusive, since the material condensed is not known, and the product of the condensation is evidenced by the uncertainty of a stain. If, as seems probable, enzymes capable of condensing the simplex carbohydrates into more complex ones exist in plants, their demonstration and study is very important to plant physiology. There is need, however, of far more conclusive methods.—WM. CROCKER.

River dunes.—A study of the vegetation of the river dunes of Illinois by GLEASON[53] proves them to be very similar to those of Lake Michigan, although differing somewhat in the sand-binding plants that assist in their formation. These are notably *Ceanothus ovatus*, *Panicum virgatum*, and *Rhus canadensis*. The first forest stage is characterized by the dominance of *Quercus velutina*, with which is associated *Q. marilandica* in the Oquawka area. The underbrush in this forest consists almost entirely of young trees of the same species. The successional stages to the maple forest are suggested, and a more exhaustive treatment of the relation of the different associations promised for the near future.—GEO. D. FULLER.

Leaf position.—BÄSSLER investigated the effect of decapitation upon the position of adjacent leaves. He reports[54] that the youngest leaves react by erecting themselves more, unless a branch has already developed in the axil, in which case the branch erects itself. The closer the leaf to the cut, the more the erection, but wounding without decapitation has no effect. Light does not influence the reaction; gravity slows it. BÄSSLER was unable to determine or analyze further the cause of the reaction.—C. R. B.

Parthenocarpy.—LONGO, in testing the effects of preventing pollination in diclinous flowers, found that a dozen flowers of *Diospyros virginiana* L., unpollinated, set fruit as well as the pollinated ones.[55] The fruits matured, but the seeds were merely coriaceous brown laminae, within whose seed coats was only the excessively developed tapetum of the embryo sac. *Diospyros Kaki* L. was already known to be parthenocarpic.—C. R. B.

---

[53] GLEASON, HENRY A., The vegetational history of a river dune. Trans. Ill. State Acad. Sci. **2**:19–26. 1909.

[54] BÄSSLER, FRIEDRICH, Ueber den Einfluss des Dekapitierens auf die Richtung der Blätter an orthotropen Sprossen. Bot. Zeit. **67**[1]:67–91. 1909.

[55] LONGO, B., La partenocarpia nel *Diospyros virginiana* L. Rendic. R. Accad. Lincei, Cl. Sci. V. **18**:632–635. *fig. 1*. 1909.